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On the Speed of the Invisible Portions of the Spectrum.

[*A Memoir submitted to the Franklin Institute for the Boyden Premium.*]

BY DR. PAUL R. HEYL.

HISTORICAL INTRODUCTION.

The question for the solution of which the Boyden Premium is offered is one that must be well understood if it is to be well answered. The vagueness of language in the original statement of the question ("all rays of light and other physical rays") is in large measure due to the incomplete and vague knowledge of the spectrum which existed fifty years ago. It may help to understand what might have suggested such a problem if we briefly review the history of the subject of radiation during the first half of the nineteenth century.

In the year 1800, our knowledge of the spectrum was substantially where Newton left it more than a hundred years before; but it was not to be long before two important discoveries were to be made which would greatly enlarge the field of study. In 1800 Herschel discovered the existence of invisible heat-rays beyond the red, and this discovery was quickly followed by the discovery of the ultra-violet rays by Ritter in 1801. A controversy over the nature of these invisible rays at once began, and continued for upwards of two generations. One school of philosophers held that there were three distinct types of rays present in light, namely, the heating, the visual, and the actinic rays. These rays were believed to overlap each other somewhat in refrangibility, accounting for the heating effect in the red and orange and for the chemical effect of the blue and violet. The other school put forth an explanation in terms of the undulatory theory, which was then having its early struggle for recognition. This explanation supposed all kinds of rays to be of the same nature, *i. e.*, undulations in an æther, but to differ somehow in quality or intensity which rendered some of them invisible. Biot was probably the earliest champion of this theory,* and defended it ably as early as 1814. Probably because the undulatory theory on which it was based was not yet generally accepted, this theory seems to have made but little headway for a time.

The progress of the undulatory theory of light may be said to have been quite encouraging by 1833, in which year Brewster, one of the last adherents of the corpuscular theory, wrote these words:†

"On these grounds I have not ventured to kneel at the new shrine, and I must even acknowledge myself subject to that national weakness which urges me to venerate and even to support the falling temple in which Newton once worshipped."

With the gradual recognition of the undulatory theory there seems to have been an increase in the support of what may be called the unitarian theory of the spectrum, and we find Ampère defending and expounding it in 1832.‡ It should be noticed that Ampère does not seem to have grasped the idea that the invisible

*Gilbert's Annalen, 46, p. 376, (1814); Rosenberger, in his Geschichte der Physik, vol. iii, p. 317, credits him with these views in 1825.

†Phil. Mag. (3) ii, p. 360, (1833).

‡Bibliothèque Univ., vol. 48, p. 225; also Pogg. Ann., vol. 26, p. 161, 1832.

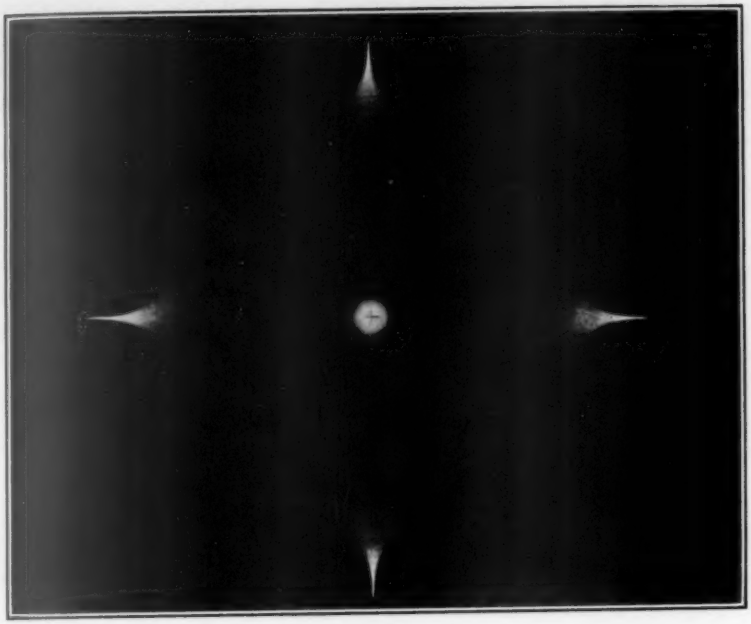

PLATE 1.
Experimental, showing appearance of spectra.

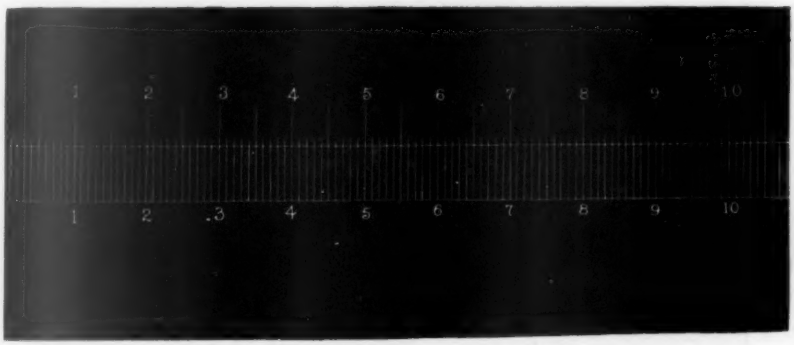

PLATE 4.
Scale of millimeters for measuring wave lengths.

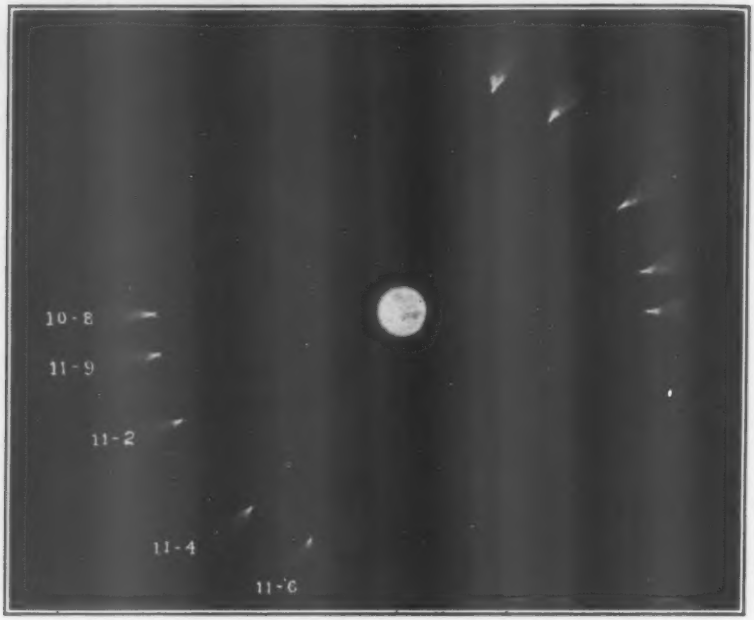

PLATE 2.
Determination of relation between focus and wave length.

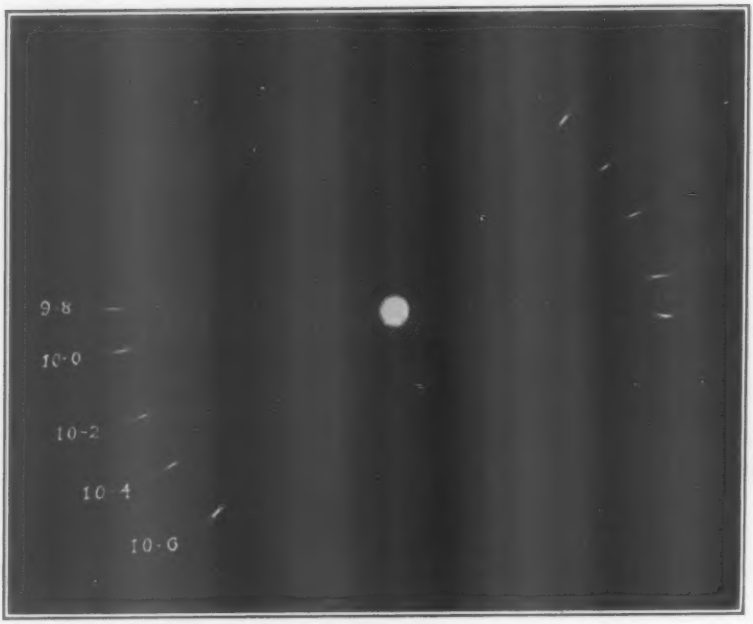

PLATE 3.
Determination of relation between focus and wave length.

waves differ in *frequency* from their visible brethren. He explained the invisibility of heat waves entirely as a question of relative intensity or abundance, and observing that water readily stopped the heat waves, concluded that it was to their absorption by the fluids of the eye that they owed their invisibility.

Melloni, who was easily the greatest authority on radiant heat at that time, combated this theory for some years, but finally capitulated, in 1842.† Yet as late as 1846‡ it was said "the identity is not yet firmly enough established."

The method which was pursued in establishing the identity of the different types of rays in the spectrum was that pointed out by Biot, in 1814. He says, in the article in Gilbert's Annalen already quoted:

"Ob der Waermestoff und das Licht blosse Modificationen eines und desselben Princips, oder zwei wesentlich verschiedene Principe sind, darüber ist seit langer Zeit unter den Physikern und den Chemikern gestritten worden. Man hat zu Gunsten der einen wie der andern dieser Hypothesen, Systeme aufgebaut; der einzige Weg indess, auf dem sich die Sache entscheiden laesst, ist, die Eigenschaften, welche dem Waermestoff, und die, welche dem Lichte wesentlich eigen sind, durch genaue Versuche mit Zuverlaessigkeit auszumitteln, ihre Aehnlichkeiten und die Verschiedenheiten beider auseinander zu setzen, und nachzuforschen, ob ein und dasselbe in seiner Natur unveraenderliche Princip, das auf unsere Organe und auf die Körper nach Verschiedenheit der Umstaende Verschieden wirkt, die mannigfaltigen Wirkungen, welche wir wahrnehmen, in ihnen hervorzubringen vermag."

It is rather remarkable that the difference of opinion as to the nature of the various rays of the spectrum seems to have found its main expression in the question as to the identity of light and radiant heat, comparatively little discussion arising over the associated question of the nature of the ultra violet rays. Much work was done in reference to the properties of radiant heat along the lines indicated by Biot, as may be seen from the following summary:

Double Refraction and Polarization by Reflection; Berard, 1812. (Mém. de phys. et de chim. de la Société d'Arceuil, tom iii,

†Comptes Rendus, 15, p. 454; Pogg. Ann., 57, p. 300.
‡Fortschritte der Physik, II, p. 166.

pp. 24-29, 46, 1812. Also Gilbert's Annalen, 46, p. 376, 1814.)

Reflection and Rectilinear Propagation had been previously shown by Mariotte. (See Biot's article in Gilb. Ann., loc. cit.)

Polarization by Transmission; Forbes, 1835. (Edin. Trans. 13, p. 131; Pogg. Ann. 35, p. 553.)

Diffraction; Knoblauch, 1846. (Pogg. Ann., 74, p. 9. Communicated to Berlin Phys. Gesellschaft on Aug. 7, antedating Fizeau and Foucault.)

Magnetic rotation; Desains and De la Provostaye, 1849. (Comptes Rendus, 29, p. 352.) These authors refer to the previous work of Wartman, 1846, who obtained some uncertain results, not confirmed by others.

Speed of Propagation; Baron F. von Wrede, 1840. (Pogg. Ann. 53, p. 602, 1841. Phil. Mag., 20, p. 379, 1842.) This article being in line with the subject of the present memoir merits some extended notice, the more so as its results are at variance with the general belief at the present day. The principle was that if light and radiant heat travel at different speeds they ought to show different aberrations. In consequence, the light and heat images of the sun in a telescope will not exactly coincide, but will be slightly displaced in the direction of the plane of the ecliptic; hence the heat from the eastern and western edges of the sun will not be equal.

To investigate this question a linear thermopile was so mounted in the focal plane of a ten-foot telescope that its edge could be moved across the field of vision by a micrometer screw. The usual eye-piece was in position behind the thermopile so that the edge of the latter could be brought by visual observation tangent to the sun's edge. With the sun's image central in the field, and using a low power, so that the whole disc was visible, the thermopile was brought tangent to the western edge, the adjustment maintained as steadily as possible for five minutes by the driving mechanism of the telescope, and readings of the galvanometer were taken continually. The tube containing the thermopile and the eye piece was then turned through 180°, so that the thermopile now became tangent to the eastern edge, and the readings of the galvanometer were repeated. A single pair of readings like this should, if all adjustments were perfect, indicate qualitatively any considerable shift of the light and heat images. To make the experiment quantitative the pile was moved toward the sun's center

by one turn of the micrometer screw, and the pair of readings was repeated. A series of measurements was made, moving the pile between each pair of readings. In this way it could be ascertained how much more the screw had to be turned on one side than on the other to give the same heat. Half of this displacement of the pile would be the shift of the two images.

Wrede seems to have made but two such complete series of measurements, only one of which was carried out under perfect conditions. During the other there were slight clouds passing now and then, and a strong wind shook the instrument. In both series, however, he believed that he found the east edge of the sun to be hotter than the west edge, which would indicate a greater aberration and a lower speed for the heat rays. The telescope was rotated on its axis between two series to eliminate any unsymmetrical absorption by the object glass. The mean result gave as the shift of the images 0.28 pitch of the screw, which was $\frac{1}{119}$ of a Swedish decimal inch. The focal length of the telescope being 101.5 Swedish decimal inches, the difference between the two aberrations was apparently

$$\tan^{-1}\left(\frac{0.28}{119 + 101.5}\right) = 4.78''$$

The aberration of visible light being a little over 20″, this result gave a speed for the heat rays of about four-fifths that of visible radiation.

This remarkable result attracted no little attention at the time of its publication. Appearing originally in a little-known Danish journal it was copied into Poggendorff's Annalen and the Philosophical Magazine. There is in the possession of the Franklin Institute a letter of Mr. Boyden's, showing that he was acquainted with Wrede's work and also throwing some light upon his construction of the prize question. Mr. Boyden had no confidence in Wrede's result, and calls his work "erroneous;" but he says that it was not his intention to limit the award of the premium to such questions as that attacked by Wrede, but to include any and all other physical rays. He says in another place that he has no evidence either for or against the existence of different rays of the same refrangibility.

Wrede's work is now forgotten, but scientific opinion to-day will share Mr. Boyden's scepticism as to its value. The fact that

it has been forgotten in spite of the publicity given it at the time would seem to indicate that Mr. Boyden but voiced the contemporary opinion of its value. The principal reasons for this distrust may be mentioned.

The constant of aberration of light is such a small quantity that any experiments on differences of this value must be capable of coarse results only. It is generally believed that any difference of speed such as the prize question suggests and Wrede sought would be of an order too small to be capable of detection by Wrede's method. The grounds for this belief will be later referred to.

Wrede's method is based upon the assumption that the surface of the sun is uniformly heated, which the phenomenon of sun spots might render us rather cautious about accepting.

The scanty observations offered by Wrede are not enough to ensure the elimination of the various errors incident to such a measurement. A slight error in centering the sun's disk, or a small change in the rate of the driving clock, or a lack of accuracy in the adjustment of the thermopile tangent to the edge of the sun would have given a false result. In particular it is to be remarked that the tangent position of the thermopile is one of the greatest delicacy; for in no other position on the sun's disk would there be so great a change in the heat received by the pile for a small displacement of the latter.

Apart from the work of Wrede no investigation of the speed of travel of the invisible rays of the spectrum appears ever to have been made up to the time of the establishment of the Boyden Premium, or indeed, up to the present time. While in many other properties the invisible rays were shown to behave in the same manner as their visible brethren, and sufficient evidence was finally amassed to satisfy the scientific world as to their identity, yet the similarity of their speeds, which naturally follows, has never yet been experimentally proven. This last piece of confirmatory evidence was lacking. True, it was not urgently needed; but may this not have been the germ of suggestion in Mr. Boyden's mind which later developed into the idea of establishing the Premium?

Let us now see what work has been done bearing on this subject since Mr. Boyden's day.

It is generally understood at present that all visible radiation

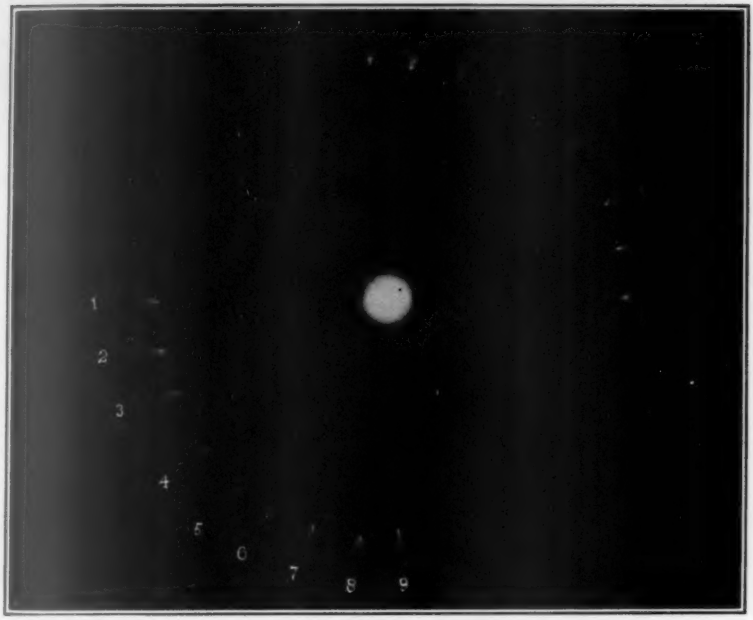

PLATE 5.
Minimum of October 13, 1905. Wave lengths, 373-383 $\mu\mu$

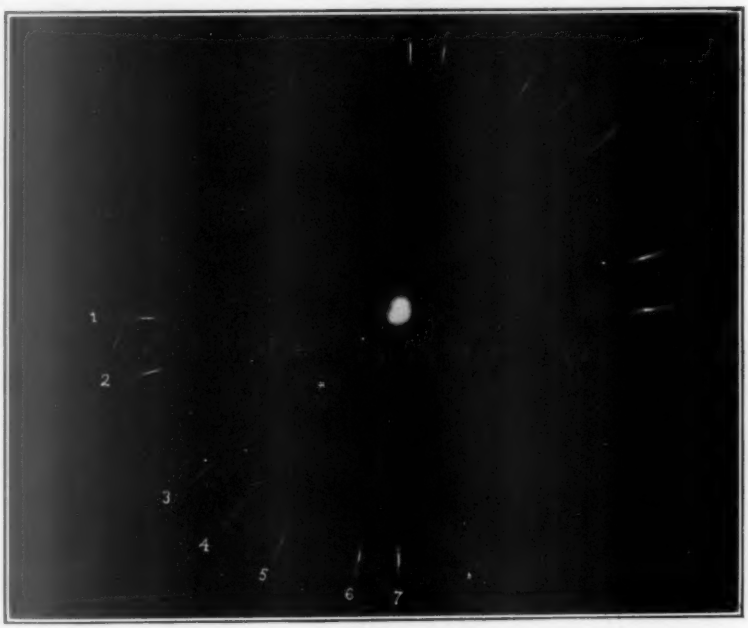

PLATE 6.
Minimum of October 13, 1905. Wave lengths, 416-468 $\mu\mu$

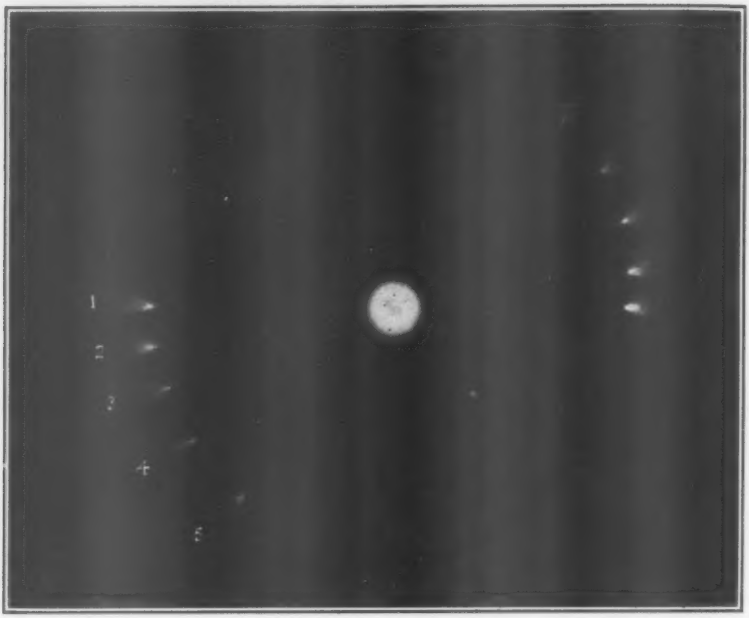

PLATE 7.
Minimum of November 2, 1905. Wave lengths, 370-378 $\mu\mu$

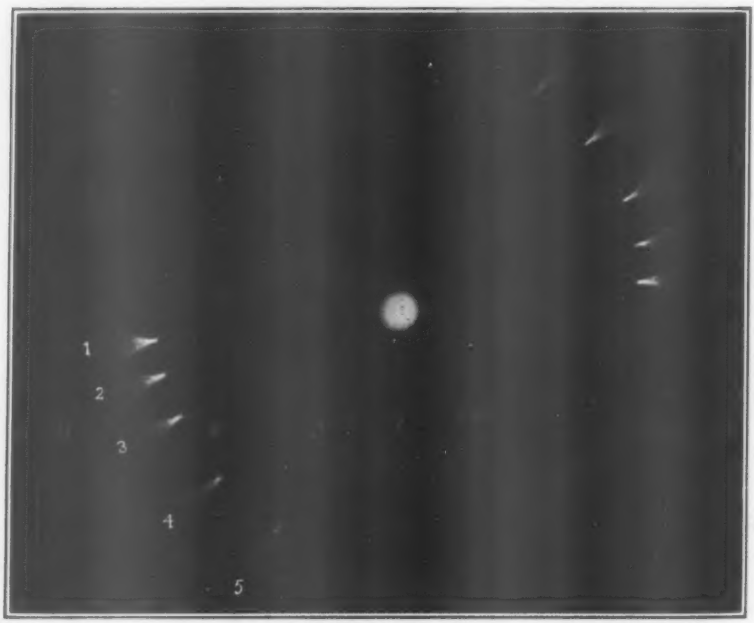

PLATE 8.
Minimum of November 2, 1905. Wave lengths, 378-389 $\mu\mu$

travels with the same speed in vacuo. This seems to have been tacitly assumed before Mr. Boyden established the Premium, probably as a mathematical consequence of the undulatory theory of light. Since that time this doctrine has not passed unchallenged by experiment, but has survived all adverse criticism, and is now quite firmly grounded.

In 1882, Young and Forbes* announced that in making some experiments on the speed of light by a modification of Fizeau's method they had observed that the luminous image was of a reddish cast while increasing in brightness and bluish while decreasing. This, they showed, could be accounted for by a difference in the speed of travel; and from their observations they deduced a difference of nearly two per cent. between red and blue light, the latter apparently traveling the faster. Shortly after this Michelson† repeated his measurement of the speed of light by Foucault's method, and paid particular attention to the question raised by Young and Forbes. He covered one-half of his luminous slit by a piece of red glass, leaving the other half uncovered, and could not detect the slightest break in the continuity of the reflected image; whereas a difference of speed of the magnitude announced by Young and Forbes would have drawn out the image of the slit to a spectrum ten millimeters wide. It seems altogether likely that the effect noticed by Young and Forbes was physiological and subjective. They may have unwittingly discovered what is now known as the Purkinje phenomenon, or at any rate something of the same nature.

All these experiments referred, of course, to the speed in air; but the refractive index from air to a vacuum is so nearly unity for all colors of light that no doubt exists as to the truth of the proposition in vacuo.

But astronomy furnishes us additional evidence for the case of light in vacuo. At the times of the eclipses of the satellites of Jupiter no change of color can be observed as the satellite disappears and reappears. Here the portion of the path of the light that lies through air is utterly insignificant compared with the length of the path in vacuo; and the distance of Jupiter is such

*Phil. Trans. part I, 1882.

†Astronomical Papers for the American Ephemeries and Nautical Almanac, Vol 2.

that a difference of speed of two per cent. between red and blue light would, as pointed out by Young and Forbes, amount to a lag of about half a minute between these two colors.

Still more conclusive is the evidence furnished by short period variable stars, none of which show any appreciable change of color in their waxing and waning, although undoubtedly at enormous distances. We may therefore conclude that the uniform speed of visible radiation in vacuo is a settled question.

This fact of the uniformity of speed of the waves making up the octave of the visible spectrum is an important one when we consider the question of a possible difference of speed in the invisible portions. All analogy with other forms of wave motion leads us to the conclusion that if the speed be constant for any considerable range of wave lengths it will also be constant, or nearly so, for wave lengths outside that range. In the case of gravitational waves in deep water, where the speed is a function of the wave length, there is no range of wave lengths, however limited, where the speed is constant; and in the case of sound waves in air the speed is constant over the nine or ten octaves that constitute the range of audibility if it is constant over a single one. For this reason we are inclined to expect the speed of the invisible rays to be nearly the same as that of the visible rays, and to distrust such a result as that announced by Wrede until it has been independently confirmed.

In view of the foregoing considerations it may be deemed a useless expenditure of time to apply to the investigation of the prize question any method except one of the most delicate description. The method about to be described by the writer may be trusted to show a difference of one part in two or three hundred thousand in the speed. Being photographic in its nature it has been found practicable to apply it only to the ultra violet and visible region of the spectrum; but as the results confirm the hitherto existing belief in the constant speed of travel so far as ultra-violet region is concerned, it becomes in the highest degree improbable that the ultra red region should exhibit any marked deviation from this speed.

PRINCIPLE AND DETAILS OF THE METHOD.

Let there be a source of white light at a great distance from the observer, and suppose this source to suffer a periodic variation in

intensity. Let the light which reaches the observer be dispersed into a spectrum by a prism or a grating. If all the different components of the white light travel with the same speed every part of the spectrum will have its maximum or minimum of intensity at the same moment; but if the speed is a function of the wave length this will not be the case.

All this, of course, is equivalent to saying that the light from the distant source will suffer a periodic change of color with its change of intensity; but in the above form the principle is applicable also to the invisible regions of the spectrum.

We are speaking in terms of Newton's hypothesis that the different periodicities of the spectrum may be considered as physically present in white light because physically present in the source. The more recent theory is that white light is due to irregular pulses in the source; that the different colors are to be considered as only mathematically present in the white light, and as physically manufactured by the prism or grating. This theory does not in the least affect the validity of the reasoning upon which this method is founded any more than it upsets the conclusions of celestial spectrum analysis. To restate the matter in terms of the pulse theory we should say that if the speed is constant for all wave lengths the pulse will travel without change of form, and if analyzed by the observer will present a simultaneous variation in intensity in all its components; while if the speed is a function of the wave length the pulse will alter its form as it travels, and when it reaches the observer it may have a differently proportioned set of components from those with which it started. As we are not concerned with the question of what periodicities are physically present in the source we shall use the Newtonian method of expression on account of its greater simplicity.

The only source of light at a practicable distance for such an experiment is a star; and such a source has the added advantage of enabling us to deal with the speed of light in vacuo, as the time taken by light to traverse the earth's atmosphere is so minute that no appreciable time lag between the different components could be introduced by the air.

We have in the star Algol (β Persei) an almost ideal source of light for such an experiment. This star shines ordinarily with a uniform lustre of about the second magnitude, but once in about three days (more exactly 2 days, 20 hours, 48.9 minutes) it suf-

fers a rapid diminution in brightness and an equally rapid recovery, occupying some seven hours in all, and involving a change in brightness of over one magnitude. This change has long been suspected to be due to a dark body revolving round Algol and eclipsing it periodically, and Vogel's measurements of the motion of Algol in the line of sight have recently put this hypothesis upon a pretty sure footing. He found that 17 hours (one-quarter of the star's period) before the minimum the star was moving away from us with a maximum speed; and 17 hours after the minimum the star was approaching us with a maximum and equal speed. This is exactly what should occur if the star were revolving around the common center of gravity of itself and a dark companion. Here, then, we have a source of light in which we can be sure that all the components have their minimum impressed upon them at the same instant.

And this source is at an enormous distance. Two attempts at measuring its parallax (by Chandler and Chase, respectively) gave the figures 0.07″ and 0.04″, the only conclusion from which is that the parallax is certainly less than 0.1″ and the distance therefore not less than about thirty light years.

This great distance enables us to make a very delicate comparison of the speeds of the different wave lengths; for suppose two waves travelled with speeds which differed by only one part in a million; then the minimum impressed upon the more rapidly travelling train of waves would reach us one millionth of thirty years, *i. e.*, about ***sixteen minutes*** before the other.

To carry out this method one must make determinations of the intensity of the different portions of the spectrum of the star, both visible and invisible, at intervals during its waning and waxing. Should all parts of the spectrum be found to have their minimum at the same moment, or, what amounts to the same thing, should the instant of minimum for every region of the spectrum be the same as the time of the visual minimum as given by the ephemeris, then the speed of travel is independent of the wave length.

In the experiments here described the comparisons of the intensities were made photographically. A suitable sensitive plate was exposed to the spectrum of the star, and a number of successive and equally timed exposures were made on the one plate, extending over a period of four or five hours, and so arranged that the

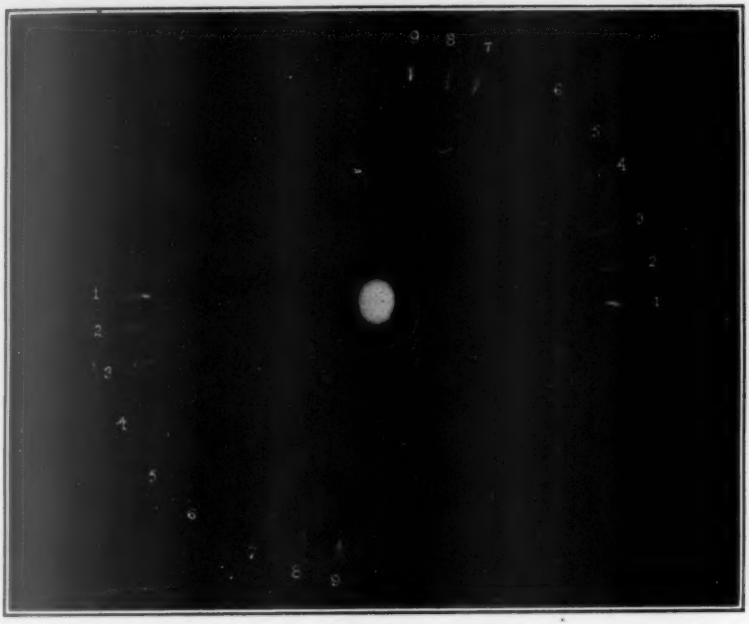

PLATE 9.
Minimum of November 5, 1905. Wave lengths, 383-400 $\mu\mu$

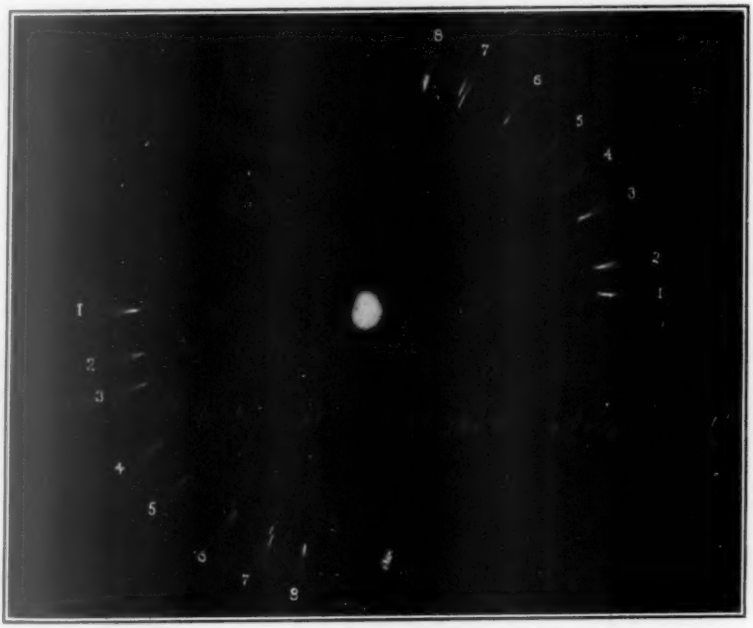

PLATE 10.
Minimum of November 5, 1905. Wave lengths, 416-468 $\mu\mu$

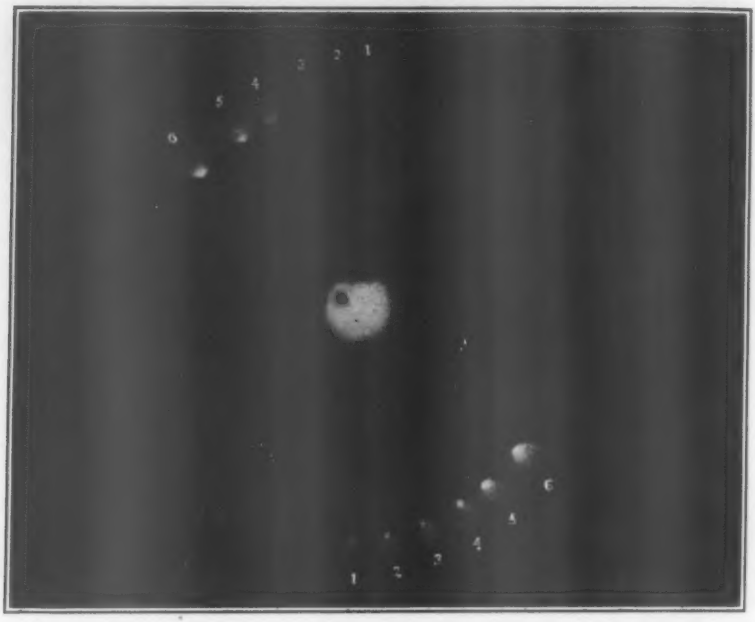

PLATE 11.
Minimum of September 5, 1906. Shortest wave length, 356 $\mu\mu$

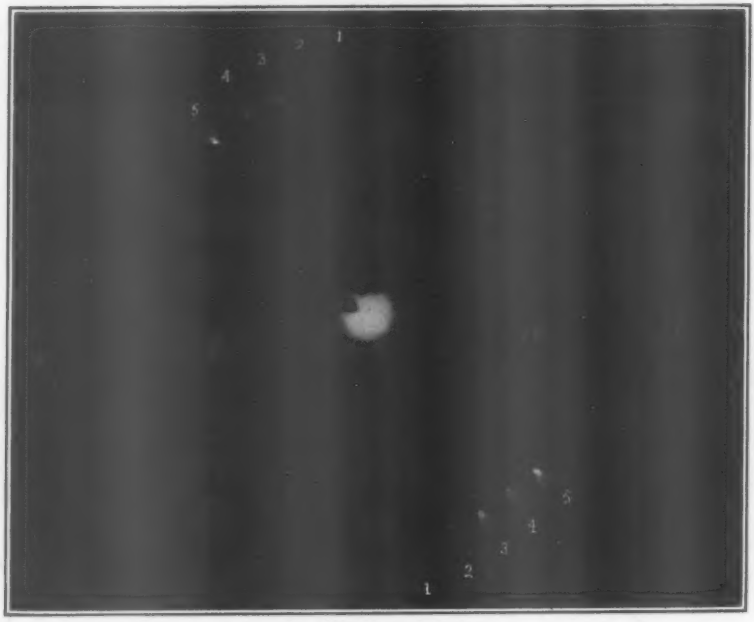

PLATE 12.
Minimum of September 5, 1906. Shortest wave length, 365 $\mu\mu$

instant of visual minimum, as given by the ephemeries, should be midway in the central exposure of the series. If, when the plate was developed, every portion of the spectrum exhibited its minimum of intensity on the same photographic image, the speed is the same for all wave lengths. Should this image be not the central one of the series, it would mean that the ephemeris was in error. The effect produced by a difference of speed of travel would be to cause the minima of intensity for different regions of the spectrum to appear on different images.

In practice it was found impossible, except for a portion of the visible spectrum, to get any great range of the spectrum in focus at one time, and many plates were necessary. Ordinary commercial instantaneous plates (Seed) were used in photographing the spectral images; and as the question was simply one of the relative intensities of the different portions of the spectrum, and the spectral lines were of no importance, the operation was a comparatively simple one.

The photographs were taken with an eight-inch equatorial telescope and an ordinary photographic camera. The lens was removed from the camera and replaced by a transparent diffraction grating (a Wallace replica) so mounted that it could be turned through any desired angle while always remaining parallel to the sensitive plate. In this way a series of radially arranged images could be taken on the same plate. The camera with the grating was attached by a light and rigid mounting to the eye-piece tube of the telescope. The optical parts are shown in schematic arrangement in Fig. 1 below.

EE^1 represents the optic axis of the telescope: the image of the star is formed by the object glass at some point A on this line. This point is in one of the conjugate foci of the eye-piece, BB^1, of the telescope; the other conjugate focus is at E^1, on the sensitive plate D. At C the grating is interposed, which produces two first order *streak* spectra at F and G. The focusing is done by altering the distance AB by moving the eye-piece tube in and out, carrying with it the camera and grating.

Plate 1* shows the appearance of the spectra thus photographed.

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*The photographic effects obtained in the negatives of the accompanying plates have been more or less emphasized in the positives for the purpose of typographic reproduction. EDITOR.

The plate is merely an experimental one and is of no other importance. Two exposures were made, turning the grating through 90° between the exposures. It will be noticed that the spectral images are not strictly streaks, but widen out fan-wise at the inner or violet end. The reason is simply that this portion is out of focus. The focus was adjusted for the visual rays, and the best that modern optical science can do is to achromatise a lens for the region from the red to the blue. From this point out into the ultra violet the focus changes rapidly for a small change in wave length. The diagram shown in Fig. II is taken from Müller-Pouillet's Lehrbuch der Physik, and shows the usual relation between wave length and focal length for the best modern achromatic combinations.

It is this lack of achromatism in the object glass of the telescope that causes the fan appearance. The object glass brings to the point A (Fig. 1) only the rays from the red to the blue. The violet and ultra violet images are to be found at various distances from A along the line EE^1. Consequently the eye-piece cannot bring them to a focus at one point. And so rapidly does the focus for these short rays change with the wave length that it is impossible to get more than a limited range in focus at once.

Plates 2 and 3 were taken with the eye-piece in different positions to ascertain where the eye-piece should be set in order to bring any particular wave length into focus. The eye-piece tube had engraved upon it a scale of centimeters and tenths, by means of which it was easy to record and reproduce the position of the eye-piece. The numbers scratched opposite the images on the plates refer to this scale.

The determination of the wave length was thus effected: The ruled surface of the grating was always 15 cm. from the sensitive plate, and carried 5684 lines to the centimeter. To determine the wave length of any point it is necessary merely to measure its distance from the undeviated central image of the star, or, what is preferable, half the distance between corresponding points on opposite spectra, and a simple trigonometrical calculation furnishes the wave length.

In measuring these distances the plate 4 is useful. It is merely an engraved scale of millimeters. The following table then gives the wave lengths:

Distance. (from center.)	Wave length.	Distance.	Wave length.
cm.	$\mu\mu$.	cm.	$\mu\mu$.
2.0	232	2.8	324
2.1	244	2.9	334
2.2	255	3.0	345
2.3	267	3.1	356
2.4	278	3.2	367
2.5	289	3.3	378
2.6	301	3.4	389
2.7	312	3.5	400
3.6	411	4.1	463
3.7	422	4.2	474
3.8	432	4.3	484
3.9	443	4.4	494
4.0	453	4.5	505

DISCUSSION OF THE PLATES.

The plates taken during the minimum of Algol may be divided into two series, those taken in 1905 and those taken in 1906. The series of 1905 includes numbers 5 to 10, and the series of 1906 the subsequent numbers. Most of the plates were developed by ferrous oxalate, to obtain the clear glass effect where there was no photographic image; but some of the plates were developed by hydrochinon, and these are easily recognized by the darker character of the background. There are but a few plates in each series because, first, the season during which the star is in a suitable position for an extended series of photographs lasts only about four months, from September to January; and second, because there are only some nine or ten minima of Algol available during the season, the others happening during daylight or so close to sunrise or sunset as to be useless; and third, one must expect these nine or ten to be still further reduced by cloudy weather. The scheme of observations is rather exacting in its weather requirements, as the star should remain unclouded for over four hours. However, a partially completed set of exposures is not without evidential value.

All the plates of the 1905 set are affected with a small error in the time of minimum as marked on the plate, a subtractive correction amounting to about six minutes for October and seven for November. The times of maximum for that year were taken from the ephemeris published each month in the astronomical col-

umn of "Nature," and it was afterward discovered that these times, although not so specified, were really *heliocentric*, that is, not corrected for the time taken by light to cross the orbit of the earth. Were Algol 90° from the ecliptic this correction would vanish; but the angle is only about 22°, and the speed of light must be taken into consideration. This error is not sufficiently large to be serious, as we shall see in discussing the separate plates. In all plates of the 1906 series the time is correctly marked. We shall consider the plates in their chronological order.

PLATES 5 AND 6. MINIMUM, 3.02 A.M., OCTOBER 13, 1905.

It was customary to expose two plates each night, each to a different region of the spectrum. All the exposures on plate 5 lasted fifteen minutes, and were arranged symmetrically in time before and after the supposed time of minimum, 3.08 A.M. The exposures on plate 6 were likewise symmetrical, but were ten minutes each. A study of the following time table, observed on this occasion, will make clear how this was accomplished.

Plate 5.	Plate 6.
1.00-1.15	
	1.17-1.27
1.30-1.45	
	1.47-1.57
2.00-2.15	
	lacking
2.30-2.45	
	2.47-2.57
3.00-3.15	
	3.17-3.27
3.30-3.46	
	3.48-3.58
4.00-4.15	
	4.17-4.27
4.30-4.45	
	4.47-4.57
5.00-5.15	

These exposures were, of course, not left entirely to the driving clock of the telescope, but were constantly guided by the help of a long finder attached to the instrument. The two or three minute interval between exposures sufficed for the performance of the following operations: turning the grating slightly, changing the plate-holder, and altering the focus. By 2.15 A.M. the star had passed the zenith, and it was necessary to reverse the telescope,

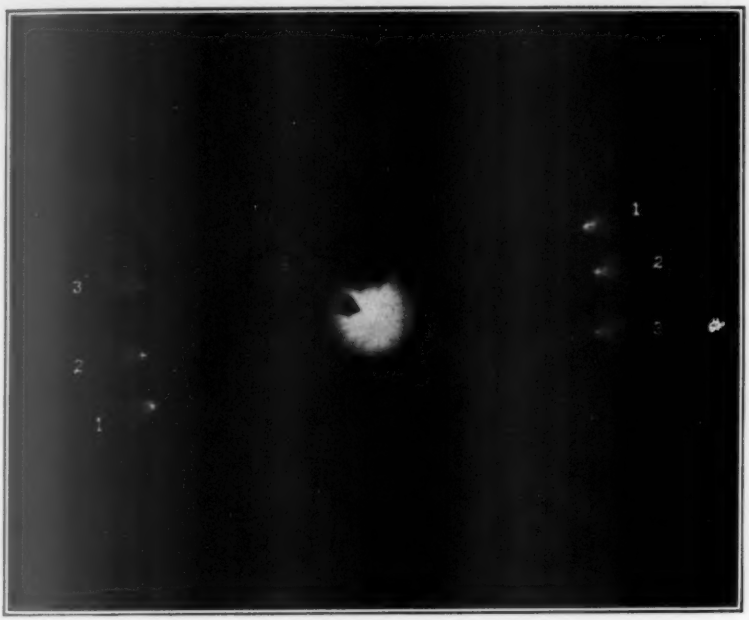

PLATE 13.
Determination of relation between focus and wave length.

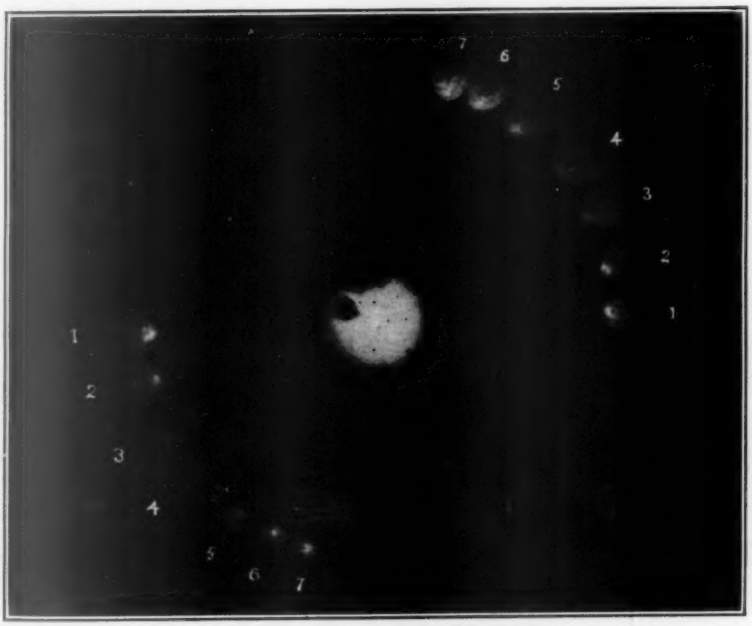

PLATE 14.
Minimum of October 15, 1906. Shortest wave length, 345 $\mu\mu$

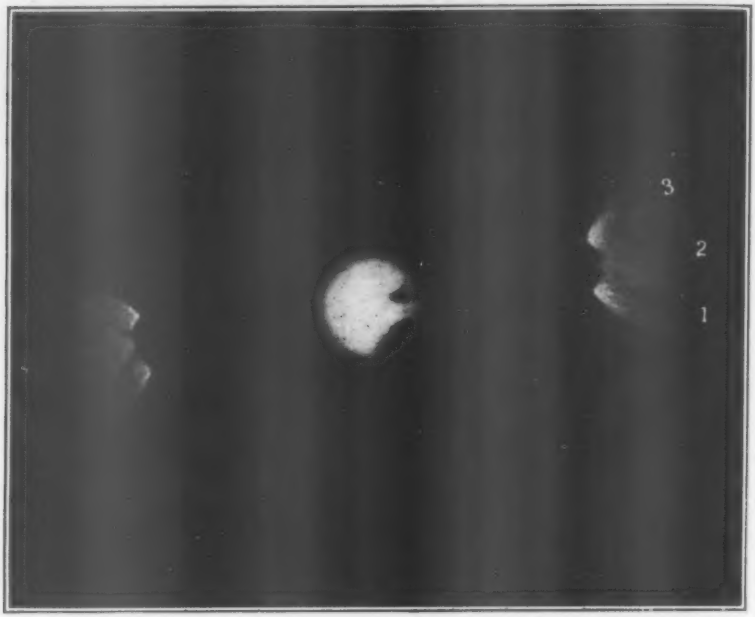

PLATE 15.
Minimum of November 7, 1906. Shortest wave length, 345 $\mu\mu$

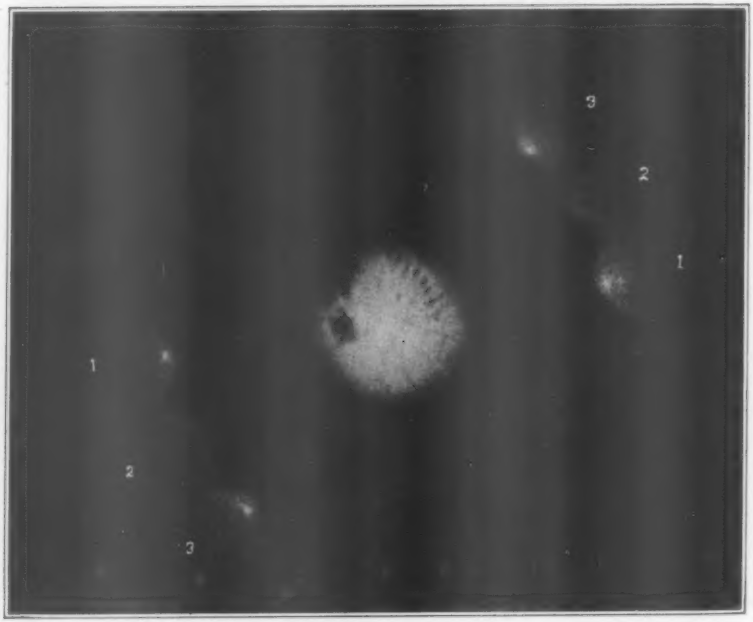

PLATE 16.
Minimum of November 30, 1906. Shortest wave length, 300 $\mu\mu$

swing it to the other side of the pillar and re-sight it. This took more time than was available in the scheduled interval, and consequently the third exposure on plate 6 had to be omitted. During the sixth exposure on plate 5 something attached to the mechanism of the right ascension regulating screw slipped out of place. The exposure was stopped at once, and the loose piece adjusted. This took nearly a minute. The exposure was started again after one minute had elapsed, and was extended one minute later than usual. This necessitated advancing the next exposure one minute to gain sufficient time for the necessary changes. The night was entirely cloudless. Occasionally a faint puff of smoke from a neighboring chimney would pass across the star, but this never occupied more than a second and happened during nearly every exposure, so that its effect on the intensity of the image may be neglected.

By looking through these plates at a white surface, not too brilliantly lighted, the best effect is obtained. It will be seen that the spectral images decrease gradually in intensity and then increase. While the fifth exposure on plate 5 is really the minimum it is impossible to detect any certain difference of intensity between this image and the ones adjoining it on either side, taken half an hour before and after. This shows that the six minute error above mentioned is a negligible quantity. Yet while we cannot with safety locate the minimum visually within a half hour the whole symmetry of the two series of images on this plate makes it certain that the minimum cannot be shifted as much as one hour from the time of the visual minimum as given by the ephemeris. As already mentioned, a difference in speed of one part of a million would produce a time lag of at least a quarter of an hour, and probably more, due to the uncertainty as to the star's distance. Consequently, the rays photographed on this plate do not differ in speed from the rays of the visible spectrum by as much as one part in about two hundred and fifty thousand. Measuring the wave lengths by means of plate 4 and the table given above it will be found that the rays on this plate are comprised within a range of from about 373 to 383$\mu\mu$. While the limit of visibility varies somewhat with different observers it may be considered to lie approximately at the violet potassium line, of a wave length 404$\mu\mu$. The rays on this plate are then entirely in the ultra violet.

In plate 6 the range of wave lengths is between 416 and 468$\mu\mu$, entirely in the visible region. The minimum is supposed to lie between the fourth and fifth images (counting the missing one) and certainly cannot be more than one hour from this. Consequently the speed of these visible rays cannot differ from that of the ultra violet rays on plate 5 by as much as one part in a quarter of a million.

There is one other important point shown by these plates. The star passed the meridian a little before the central exposure of the series, and had atmospheric absorption any sensible effect the intensities of the images after minimum would have been rendered more nearly uniform than those of the first half of the series. We may conclude, then, that for the purpose of this method a distance of as much as three hours from the meridian exerts no effect upon the intensity of the image that is comparable with that produced by the star's own change in brightness.

PLATES 7 AND 8. MINIMUM 4.44 A. M., NOVEMBER 2, 1905.

This minimum happened so near the morning twilight that only the first or waning half was available. The following is the time table, supposing the minimum to have been at 4.51 A.M.:

Plate 7.	Plate 8.
	2.13-2.28
2.30-2.40	
	2.43-2.58
3.00-3.10	
	3.13-3.28
3.30-3.40	
	3.43-3.58
4.00-4.10	
	4.13-4.28
4.30-4.40	

An exposure on plate 8 was started at 4.43, but a faint haze came over the sky almost at once, and the series was terminated. During the rest of the night the weather conditions were perfect. On both plates the images decrease steadily until the end, and we may safely assert that the minimum for the rays there represented does not happen as much as one hour *earlier* than the ephemeris time; consequently the speed of these rays cannot be *greater* than that of the visible rays by one part in a quarter of a million. The rays on plate 7 are between 370 and 378$\mu\mu$, and those on plate 8 between 378 and 389. Both plates deal entirely with the ultra

violet. On this date the star crosses the meridian about half past twelve.

PLATES 9 AND 10. NOVEMBER 5, 1905. MINIMUM, 1.32 A. M. CULMINATION, ABOUT 12.15 A. M.

The time table for these plates was as follows:

Plate 9.	Plate 10.
11.31-11.46	11.49-11.59
12.01-12.16	12.19-12.29
12.31-12.46	12.49-12.59
1.01- 1.16	1.19- 1.29
1.31- 1.46	1.49- 1.59
2.01- 1.16	2.19- 2.29
2.31- 2.46	2.49- 2.59
3.01- 3.16	3.19- 3.29
3.31- 3.46	

In the above time table the minimum was supposed to occur at 1.39 A.M.

These plates show certain irregularities which are not to be accounted for by passing clouds, as the weather conditions were apparently perfect. In plate 9 it will be noticed that the images numbered 7, 8 and 9 are very much darker than number 1, although in the latter the star was near the zenith and in the former about two hours past it. Moreover, the minimum is much more difficult to detect than in any of the preceding plates. It is certain, however, that 1 is darker than 2. This may be seen to better effect by looking through the plate at a gas flame covered by an opaque globe, such as covers Welshbach mantles, and holding the plates so that the images lie a little off the globe and exhibit an opalescent effect. The minimum is theoretically at number 5, but should it be shifted as far forward as 3, and more than this cannot possibly be claimed, the difference in speed could not exceed one part in a quarter of a million. The range of wave lengths on this plate is from 383 to 400 $\mu\mu$.

In plate 10 very much the same conditions prevail; in addition,

6 is certainly much fainter than 5, although the brightness of the star is on its upward path. It happens, however, that a previous plate (number 6) covers the same range of wave lengths as shown in plate 10, and gives a regular minimum. It must be, therefore, that the irregularity referred to was the result of some disturbance peculiar to the evening on which plate 10 was taken, and of a nature not to be detected by the eye. It may have been some change in humidity or temperature in the upper layers of the atmosphere which produced a weakening of the spectrum at that point and that time. Possibly the very strongly marked final images and the obscure minimum on poth plates may have been due to a progressive increase in the transparency of the atmosphere for the violet and ultra-violet rays during the night. It is obviously impossible to follow such changes by observing the atmospheric conditions in the neighborhood of the telescope, as observations in order to be effective would have to be made all the way up to the top of the atmosphere.

PLATES 11 AND 12. MINIMUM 12.06 A. M., SEPTEMBER 5, 1906. CULMINATLON, 4 A. M.

The dark background of these plates is due to their having been developed by hydrochinon. The early part of the night was cloudy, and but little more than the last or waxing half of the eclipse could be photographed. The time table follows:

Plate 11.	Plate 12.
11.30-11.42	
	11.45-11.57
12.00-12.12	
	12.15-12.27
12.30-12.42	
	12.45-12.57
1.00- 1.12	
	1.15- 1.27
1.30- 1.42	
	1.45- 1.57
2.00- 2.12	

On both plates there is readily recognized a well marked increase in the intensity of the images after the first two. The minimum consequently cannot be as much as one hour *later* than the minimum of the visual rays, and the speed cannot be *less* than that of the visible rays by as much as one part in a quarter of a million.

The dark background makes it difficult to measure wave lengths on plate 11, but plate 12 is clearer, and the shortest wave lengths reached is about 365 $\mu\mu$. Plate 13 (about to be described) tells us that the shortest wave lengths reached on plate 11 is about 356$\mu\mu$.

PLATE 13.

This is merely an experimental plate, exposed for half hour intervals at three different foci, to determine the wave lengths reached. It will be noticed that the third exposure, reaching a wave length of about 345$\mu\mu$, is very much fainter than the two others. Either there is an absorption band in this region or we are approaching the end of the star's spectrum. The exposure in each case was half an hour.

PLATE 14. MINIMUM OF OCTOBER 15, 1906, 3.29 A. M. CULMINATION, ABOUT 1.30 A. M.

But one plate was taken on this occasion, on account of the greater length of exposure necessary. The time from 1.44 to 5.14 was divided into seven half-hour exposures, central about the time of minimum. Through an accidental displacement of the grating the fourth or central exposure was lost, and the plate shows only the three before and the three after minimum. Very little may be safely gathered from the images on this plate; the images are all too faint, and the minimum is not all definite. The shortest wave length reached is about 345$\mu\mu$. This region of the spectrum was rephotographed on the next available occasion with a longer exposure, and is shown in

PLATE 15. MINIMUM OF NOVEMBER 7, 1906, 2.00 A. M. CULMINATION ABOUT MIDNIGHT.

The time table for this plate was as follows:

	11.00-12.00
	1.30- 2.30
•	4.00- 5.00

central about the time of minimum. It will be noted that in addition to increasing the length of the exposure the first and last exposure are placed somewhat farther from the minimum than was the case with any of the preceding plates. The middle of the

terminal exposure is in each case two and a-half hours from the minimum, while in the previous plates it never exceeded two hours. This was, of course, to obtain a greater difference in the intensities of the end and central images.

In examining this plate the first thing that will be noticed is that the images on the right are much darker than those on the left. This may have been caused by using a portion of the grating which threw more light to one side than to the other. There is a very definite minimum evident; in fact the central exposure cannot be seen at all on the right, and can be detected only with difficulty on the left. In looking for this image it will aid in its detection if one notes that the images are somewhat parabolic in shape with a dark nucleus at the vertex. On the left side, the "comet's tail" of number 3 is considerably darker where the tail of number 2 intersects it, and having found this point it is easy to see the nucleus of 2. We may conclude that the minimum for these wave lengths cannot occur as much as one hour from the time of minimum of the visible rays, else the central image would be about equal in intensity to one of the end ones. The visible transparency of the atmosphere was perfect throughout the night. Certain fifth magnitude stars in the neighborhood of Algol were used as tests for this, and were plainly visible at all times during the exposure. We may say then that the speed of ultra violet light as far out as 345 $\mu\mu$ does not differ from the speed of the visible rays by as much as one part in a quarter of a million.

PLATE 16. MINIMUM OF NOVEMBER 30, 1906, 12.31 A. M. CULMINATION, ABOUT 10.20 P. M.

Three exposures were made on this occasion as follows:

10.01-11.01
12.01- 1.01
2.01- 3.01

The focus was adjusted so as to reach rather far into the ultra violet, and the plate shows a wave length of about 300 $\mu\mu$. The images are extremely faint, but can be located by aid of the numbers scratched on the plate. The images are shaped like parabolas, and may best be seen by viewing the plate by reflected light against the background. If a gas flame is used the reflection of the flame should appear on the plate a little to one side of where

the images are supposed to be. The number lies between the images and the central image of the star.

The images on this plate exhibit a well-marked minimum, although the first of the three is in each case somewhat lighter than the third. The middle of the terminal exposures being two hours from the time of minimum, we may safely say that the minimum for the rays here represented cannot have been shifted by as much as one hour from the time of minimum of the visible rays, else the central image would be as dark as one of the end ones; consequently there cannot be a difference of speed as great as one part in a quarter of a million.

RESUMÉ.

The plates submitted with this memoir cover (somewhat discontinuously) the region of the spectrum lying between wave lengths of 468 $\mu\mu$ in the blue to 300 $\mu\mu$ in the ultra violet. The end of the visible spectrum is assumed to lie at about 400 $\mu\mu$. The speed of the rays within this range is constant to one part in a quarter of a million.

It has long been recognized that the speed of the visible rays of the spectrum is constant to a high degree of accuracy, but so far as the author is aware there has never been heretofore any experimental determination, either absolute or relative, of the speed of the ultra violet rays. Their speed, as well as that of the ultra red rays, has on theoretical grounds been believed to be the same as that of the visible rays. The experimental confirmation of this belief for the ultra violet rays adds weight to the generally received opinion as regards the ultra red end of the spectrum.

A FEW WORDS CONCERNING THE ULTRA RED RAYS.

The method described in this memoir being photographic in its nature it would seem hopeless to attempt to apply it to the ultra red portion of the spectrum; yet this matter was carefully tested. As much as twenty years ago plates were made by Abney which were sensitive as far as 1000 $\mu\mu$, although a very long exposure was required for this.

The preparation of these plates is unusually difficult, and they do not keep well. Those who have attempted to repeat Abney's experiments have usually failed, and the author forms no excep-

tion to this statement. However, Lehmann* has of recent years published a method of obtaining with comparative ease a plate which is sensitive as far into the ultra red as Abney's. He stains an ordinary instantaneous plate in an ammonical solution of nigrosin and alizarine blue S, containing a small amount of silver nitrate. The author has carefully tested Lehmann's formula and succeeded in reaching a wave length of 1000 $\mu\mu$ with a Welsbach mantle as the source of light; but the attempt to use these plates on the stars showed at once that the exposure required would be prohibitive, occupying in fact more than a whole night.

It might be possible with a rotating mirror apparatus, such as used by Michelson and Newcomb, and a bolometer to locate the ultra red image, to obtain a result for the ultra red rays accurate to one part in a thousand, possibly; but, as has been pointed out in the early part of this memoir, it would not be worth the great expenditure of time and trouble to apply to the solution of this point a method whose results were of so approximate a nature.†

Appendix A.

The Boyden Premium.

In 1859, Uriah A. Boyden, Esq., of Boston, Mass, deposited with the Franklin Institute the sum of one thousand dollars, to be awarded as a premium to "Any resident of North America who shall determine by experiment whether all rays of light, and other physical rays, are or are not transmitted with the same velocity."*

The following conditions were established for the award of this Premium:

1. Any resident of North America, or of the West India Islands, may be a competitor for the Premium; the southern boundary of Mexico being considered as the southern limit of North America.

2. Each competitor must transmit to the Secretary of the Franklin Institute a memoir describing in detail the apparatus, the mode of experimenting, and the results; and all memoirs received by him before the first day of January, one thousand nine hundred and eight will, as soon as possible after this date, be transmitted to a Committee of Judges.

3. The Board of Managers of the Franklin Institute shall, before the first day of January, one thousand nine hundred and eight, select three citizens of the United States of competent scientific ability, to whom the memoir shall be referred; and the said Judges shall examine the memoir

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*Arch. für Wissensch. Photog. II, 216, 1900. See also *Baly's Spectroscopy.*

†The author, now that his identity is disclosed, desires to record his obligation to his colleague, Prof. M. B. Snyder, for placing at his disposal the telescope of the Central High School.

and report to the Franklin Institute whether, in their opinion any, and, if so, which of the memoirs is worthy of the premium. And, on their report, The Franklin Institute shall decide whether the Premium shall be awarded as recommended by the Judges.

4. Every memoir shall be anonymous, but shall contain some motto or sign by which it can be recognized and designated, and shall be accompanied by a sealed envelope, endorsed on the outside with some motto or sign, and containing the name and address of the author of the memoir. It shall be the duty of the Secretary of the Franklin Institute to keep these envelopes securely and unopened until the Judges shall have finished their examination; when, should the Judges be of opinion that any one of the memoirs is worthy of the Premium, the corresponding envelope shall be opened, and the name of the author communicated to the Institute. The sealed envelopes accompanying unsuccessful memoirs will be destroyed unopened, in the presence of the Board of Managers.

5. Should the Judges think proper, they may require the experiments described in any of the memoirs to be repeated in their presence.

6. The memoirs presented for the Premium shall become the property of the Franklin Institute, and shall be published as it may direct.

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*The problem has been more specifically defined by the Board of Managers as follows:

"Whether or not all rays in the spectrum known at the time the offer was made, namely, March 23, 1859, and comprised between the lowest frequency known thermal rays in the infra-red, and the highest frequency known rays in the ultra-violet, which in the opinion of the Committee lie between the approximate frequencies of 2×10^{14} double vibrations per second in the infra-red, and 8×10^{14} in the ultra-violet, travel through free space with the same velocity."

Appendix B.

Report of the Committee of Judges

On the foregoing Memoir submitted by the author under the pseudonym "Algol."

To the President and Members of The Franklin Institute:

Regarding the application by "Algol" for the Boyden Premium, we respectfully report that the applicant has demonstrated by experiment that the velocities of the visible and the ultra-violet rays are equal, the method of observation yielding such a degree of accuracy that the maximum of possible error cannot exceed one part in two hundred and fifty thousand. This degree of accuracy the Board of Judges deems to be much in excess of that usually expected in problems of this character. The applicant has accordingly furnished the required proof as regards the relation of the visible and the ultra-violet rays. The experimental proof as regards the infra-red rays remains still lacking, the method adopted in this memoir not being suited for those rays.

The Committee, however, considers the work presented to be of such a high character as to be eminently worthy of distinct recognition and

strongly recommends that the applicant be awarded the sum of One Thousand Dollars ($1,000) from the accumulated Boyden fund, the surplus being retained by the Institute to be awarded to any one who may in future solve the same problem in regard to the infra-red rays.

The Judges desire to reserve the right of witnessing one of the experiments if in their judgment they deem it desirable after learning the name of the applicant.

(Signed)

HUGO BILGRAM,
ARTHUR W. GOODSPEED,
GEORGE F. STRADLING.

Philadelphia, Pa., June 12, 1907.

To the President and Members, Franklin Institute:

The Committee of Judges, after learning the name of the applicant referred to in preceding report, has unanimously decided that any further experimental demonstration of the problem is unnecessary.

(Signed)

HUGO BILGRAM,
ARTHUR W. GOODSPEED,
GEORGE F. STRADLING.

Philadelphia, Pa., June 14, 1907.

APPENDIX C.

At the stated meeting of the Franklin Institute held Wednesday, June 19, 1907, the following resolution of the Board of Managers was reported, viz.:

> "*Resolved*, That the Board of Managers hereby recommends to the Institute the approval of the report of the Judges upon certification from the Judges that they consider any further experimental demonstrations unnecessary." (A certification to this effect, signed by all the Judges, was found appended to the Judges' report.)

A motion was thereupon unanimously passed, confirming the award as recommended by the Judges and endorsed by the Board of Managers at its stated meeting held Wednesday, June 12, 1907.

WM. H. WAHL, *Secretary.*

Section of Physics and Chemistry.

(Stated Meeting held Thursday, October 11, 1906.)

A Résumé of the Literature of the N Rays, the N_1 Rays, the Physiological Rays and the Heavy Emission, With a Bibliography.*

GEORGE FLOWERS STRADLING, PH.D.

(Continued from vol. clxiv, p. 74.)

June, 1904.—Blondlot[42] takes up the thread of his work, saying that Becquerel's discovery of the brightening of the phosphorescent screen being due to the effect of the N rays on the eye and not on the screen shows why photography cannot be used to show the brightening of the screen. He contends, however, that the brightening of the electric spark is not due to the action of N rays on the retina, because the interposition of water causes no change in the appearance of the spark. The effect on a heated sheet of platinum is also unchanged by the interposition of water.

Blondlot[43] announced his discovery of a new property of matter, that of projecting spontaneously and continuously an emission subject to gravity and detected by its brightening a Ca S screen. A coin is held at any distance vertically over a screen. The latter grows brighter, provided both screen and coin are horizontal. If, however, the coin is beneath the screen and more than 6 cm. away the effect fails to appear. Copper, zinc, lead, wet cardboard acted as the coin did, while gold, platinum, glass, dry cardboard did not show the effect.

When the coin was put in a vertical plane the effect of brightening was found along two curves, one on each side of the coin. They seemed not to be parabolas.

*Numerals above the line refer to the numbers of papers in the bibliography at the end of this article.

The emission passes through a sheet of paper or cardboard and even a plank 2 cm. thick. A pane of glass stops it, by causing it to rebound.

A fortnight later Blondlot[45] states that a magnetic field deflects the jet of the ponderable emission, and that it appears to resemble an electric current. The original jet was separated by the magnetic field into three, one of which showed no electrical charge, the remaining two showing positive and negative charges respectively. The two jets acted on by the magnetic field were deflected by an electric field also, the directions of deviation in the two cases being consistent. The ponderable emission is carried along by air currents, and resembles the N rays in its action on the electric spark.

In a paper of the same date the same physicist[44] announces some small improvements in the photographic method of registering the effect of the N rays on the electric spark. He introduces an aluminium lens to concentrate the rays upon the spark.

Five papers appeared from the pen of Charpentier this month. He finds[80,94] additional reason for asserting that a Ca S screen, mounted upon an object which affects one of the senses, grows brighter when brought near a part of the brain especially connected with that sense.

One paper[93] is practically the same as that of one of the previous month.[79]

He finds that thermal sensibility is increased by the N rays and lessened by the N_1 rays. Put cold water in a test tube and pour hot water on top. Put the finger on the outer surface of the tube where the glass feels hot. The approach of a source of N rays makes the glass feel hotter. When the finger is in contact with a cold object the sensation is made more intense by the N rays. He thinks the N rays produce an increase of tactile discrimination.[95]

Since both sonorous and nervous vibrations act on the phosphorescent screen, they will produce a greater effect, if their periods are the same. Charpentier uses this to determine the frequency of nervous vibrations, finding it to be about 830[81,96] *per sec.*

Colson[104] continued his investigation of the production of N rays by chemical action.

Gutton,[108] who had previously investigated the effects of the magnetic field and of electric waves on the phosphorescent screen,

now contributes a paper bearing exclusively upon the N rays. A spectrum was formed of light from which the N rays had been removed. The violet end appeared brighter when N rays fell either upon the eye of the observer or upon the surface receiving the spectrum. This effect decreased from the violet toward the red end, disappearing at the red. Moreover the visible spectrum was prolonged toward the ultra-violet.

Becquerel[10] proceeds with his study of anæsthetics. He found that heating a body develops N_1 rays when expansion is produced, but N rays when contraction follows.[11] Prince Rupert drops emit N_1 rays parallel to the length, and N rays perpendicular to this direction. Alcohol has less effect on emission than chloroform.

He finds that both N and N_1 rays cease to act on the Ca S screen after they have passed through a magnetic field in a direction perpendicular to the lines of force. Passage along the lines of force makes no difference. He unearths analogies between the action of radium and of the N rays and concludes that the N and N_1 rays consist of an undulatory effect and of a material emission combined.[12]

Bichat[23] found that connecting to earth a source which is producing fluctuations of the brightness of a screen causes the variations to cease (vid. 143 and 166). N rays are obtainable merely by joining a metal plate to the earth.

Quartz held with its axis perpendicular to the screen causes brightening; when, however, the axis is parallel to the screen, the latter becomes darker.[24]

The opposite is true in the case of Iceland spar. This relation was found to hold for several positive and negative crystals.

E. Meyer and Lambert[128] found that N rays are emitted at the moment when blood coagulates.

Julien Meyer[131] states that, when a file as source of N rays is placed beneath a vessel of pure water above which a Ca S screen is held, the latter becomes less luminous. When the water is removed it grows brighter. From this he concludes that water emits N_1 rays under the action of N rays. Adding salt to the water is followed by the disappearance of the effect. Water has no reversing effect upon N_1 rays.

Three new contributors to N rays literature appeared in June, 1904.

Mercanton and Radzikowski[174] subjected the nerves of a frog to N rays but could discover no change in their electrical resistance, activity or rapidity of reflex action.

Rothé[137] develops a photographic method of studying the effect of the N and N_1 rays upon the phosphorescent screen. A circular patch of Ca S is put at a fixed distance from the photographic plate and exposure is made for five seconds. Then after twenty seconds a second exposure on a new part of the plate is made again for five seconds, and so on. The images so produced are not alike since the phosphorescent material emits less light as time passes. The diameters of the images were found to vary regularly with the time. When N rays acted upon the Ca S Rothé claims that the regularity disappeared owing to the screen becoming more luminous. His published data hardly seem to warrant this conclusion. It is also to be remembered that more than a month earlier Becquerel[8] had claimed to show that the N rays do not really increase the luminosity of the screen, the effect observed being due to the action of the rays upon the retina of the observer.

During the month of May criticism did not find its way into print. This was not the case in June.

Le Roux[169] states that there are apparent variations in brightness when a feebly illuminated surface is looked at in a darkened room. These variations he holds to be subjective. If a person looks at a Ca S screen it grows darker after half a minute. There comes a time when obscure clouds seem to pass across the luminous patch. Other people see oscillation of brightness. The initial aspect can be regained at any time by exposing the eye to white light, by coughing or by making noises.

The smelling of chloroform or ether reduces the brightness of the screen if the nostril on the side with the observing eye is used. The other eye is not affected. The inhalation of alcohol on the contrary produces a brightening of the screen.

Pacini[176] in Rome reported to Nature that he failed to get Blondlot's results.

The Revue Scientifique[199] threw out the warning, "It is to be feared that M. Charpentier has perceived only the variations of attention excited by the approach of a body or provoked by the suggestibility of the subject. The Academy of Sciences would do well to demand experimental demonstration. Radium has

made it easy to believe. Does not the effect of the N rays resemble that of magnets on hysterical subjects?" Doubt is again expressed a fortnight later.[200]

Burke[148] again reports failure in striving for Blondlot's results. He thinks expectation and concentration of attention are important factors in success. He could not detect the photographic registration of the effect of the N rays on the spark.

Baumhauer[144] failed to get any effect of the N rays upon a screen of Sidot's blende.

Salvioni[183] performed a lengthy series of experiments. "For several months I have dedicated myself with much constancy and little success to the phenomena recently described by Blondlot." He sought in vain for objective effects, using in turn the coherer and photoelectric methods of detection. When he turned to the phosphorescent screen he obtained some results similar to those of Blondlot. When he studied the foci behind a lens, the more carefully he worked the more of them he found until within a score of cm. there seemed to be 50 or 60 foci.

He holds that many of the results point to subjective causes, while on the other hand there are indications that along with the subjective there is something objective. In his opinion the phenomena of Blondlot have a singular resemblance to the pretended luminous phenomena of animal magnetism.

July, 1904.—This month witnessed the appearance in N ray literature of several well known names for the last time,—Becquerel, Bichat, Charpentier and J. Meyer.

Becquerel[13] claims that β rays cause a calcium sulphide screen to emit N rays. Polonium darkens the screen. He compares further the effects of β and N rays and of α and N_1 rays.

There is a difference, he finds,[14] between N rays from a Nernst lamp and those from tempered steel, Prince Rupert drops, compressed wood and Ca S previously exposed to the sunlight. Rays from the lamp are not affected by traversing a magnetic field perpendicular to the lines of force, while the other rays are changed in effect. He holds the N rays to consist of three kinds:

1. Such as are not deviated by the magnetic field and which do not act on Ca S.

2. Such as are deviated by the magnetic field and are much dispersed, like the β rays.

3. Such as are deviated in the opposite direction, as are the α rays.

The number of rays into which a beam of N rays is dispersed by the magnetic field varies with their source. He finds not only qualitative but even quantitative agreement between the effects of the magnetic field upon the N rays and upon the β rays.

In his final paper[15] Becquerel claims that the non-deviable rays are those which give the eight rays by refraction through an aluminium prism. When the N rays pass through air or copper they produce α rays perpendicular to their path, while under similar conditions N_1 rays produce β rays. He finds the nature and effects of the Blondlot rays to be more complex than does any other experimenter.

Bichat[25] found that the rays from a platinum wire, rendered incandescent by the current from a storage battery, after traversing aluminium make a Ca S screen brighter; while, if the electrical circuit is entirely insulated, the screen grows darker. Then joining any point of the circuit to the water pipes again causes brightening. Other curious effects of connection with the earth are described.

In the case of the heavy rays acted on by gravity they make the screen darker if their source is insulated. Joining the source to earth or electrifying the insulated observer causes an increased brightness.

When a person stands facing a wall Charpentier[82] finds that a Ca S screen shows positions of maximum and of minimum brightness when moved along from the body to the wall, indicating stationary waves of wave-length 3.5 mm. In one experiment 14 maxima were located in a distance of 52 cm. The wave-length is the same as that of waves in nerves determined by Charpentier by another method. In front of the eye the waves were only 2 mm. long. Other sources of N rays also gave standing waves in air.

In his closing paper Charpentier[97] discusses certain new forms of screens.

A careful statement of the conditions of success in N ray work is given by Le Roux.[120] He is very confident of his ground, saying: "I have been able to attain not merely to confirming the variations of brightness announced, but to seeing them under

conditions such that they cannot be tainted with any suspicion of subjectivity."

Blondlot[47] sets forth a new method of observing his rays. A longitudinal streak of Ca S is put on white paper and illuminnated by orange light so regulated as to cause the bluish phosphorescent light to disappear. When N rays fall upon the calcium sulphide it becomes visible again. When the orange light barely permits the screen to be seen N_1 rays cause it to disappear.

He returns to his study of the heavy emission,[46] and announces that cleaning a piece of metal makes its emission cease. Heating it to 100° and letting it cool restores the property. No amount of cleaning seems able to stop the emission of lead, yet long exposure to the air brings it about.

All the liquids examined gave the heavy emission. The following are inactive: Platinum, iridium, palladium, gold and dry glass.

The heavy emission also received the attention of J. Meyer[132] in his final paper on the subject. Parts of the human body, he affirms, send out the heavy rays. He investigated the phenomena of transmission and absorption. When the hand is held over a funnel the emission collects in a bottle placed beneath, and can be poured out upon a screen which it causes to light up. It can be kept in an open bottle for several days. He concludes that different jets of this emission vary in speed.

Colson[105] is lead to announce certain conclusions concerning the constitution of dissolved salts from considerations based in part upon N ray effects.

Raoult[135] of Nancy reports his experiments confirming Charpentier's observations that the N rays increase acuteness of hearing while the N_1 rays produce the opposite effect.

Never at any time did the N ray investigators advance with more confidence or feel firmer ground beneath their feet than in this month of July, 1904, and yet their work ceased with remarkable abruptness. During August, September and October there appeared not a single paper recording investigation.

The Revue Scientifique [201,202] raised its voice in warning again. It criticized Charpentier's work in relation to the effect of the N and N_1 rays upon the sensation of temperature. He should not, it asserts, have made the tests upon himself but upon a

bandaged subject, kept ignorant of the approach and withdrawal of the alleged sources of the rays.

Later in the month in speaking of Salvioni's work[203] editorial comment was made upon those experimenters who "observe with a disconcerting facility properties more or less marvellous, whose origin ought to be sought much less in the N rays than in the depths of their subconsciousness, or in the action of badly interpreted natural phenomena."

During the months of August, September and October there was no lack of criticism. At the *Versammlung deutscher Naturforscher und Aerzte*[196] at Breslau in September a discussion on the N rays was held. Lummer and Rubens reported that they had obtained no results in repeating the most important of Blondlot's experiments. They failed to get the photographic effect with the spark. Blondlot sent some of his plates to Lummer, whereupon they tried once more and failed as before. They found that the brightness of a fluorescent screen changed in the ratio of 1 to 4 merely by moving the eye.

Paul Weiss, of Zurich, stated that he had noted changes in the brightness of the screen when moved over the nerves of the wrist, but that the changes were fitful. For three months, however, he had tried this and got the same topography of the nerves, even when he tried to get something different. He seemed to be of the opinion that the effects might be objective.

The discussion at the meeting of the British Association for the Advancement of Science[197] was very one-sided. Lummer reported the failure of Rubens and himself to get the results sought. Burke told that he had tried various persons but had found no evidence of external action upon the sight.

At the sixth International Congress of Physiology[198] at Brussels, Sept., 1904, Lambert, in discussing his researches on fermentation, came to speak of the N rays. The German members expressed their disapproval by absenting themselves at the time. At the close of this paper Henri stated that long and numerous experiments had been made on the N rays at the laboratory of physiology of the Sorbonne, but with no success. Pieron told of equally fruitless researches at the laboratory of physiology of the Hautes-Etudes. Then a series of physiologists from countries other than France rose and told of equal lack of success,—Querton of Brussels, Herzen of Lausanne, whose assistant went

in vain to Charpentier, and representatives of Italy, Russia and England.

Waller, the eminent physiologist of the nervous system, leaned toward one of his neighbors and said that the rays of Nancy should have been called the rays of suggestion, alluding to the studies in obscure mental phenomena made at that city.

Pieron asked Lambert to submit to the test of looking at the screen and telling when a source of N rays was brought near. The latter however excused himself on the ground of fatigue.

R. W. Wood,[193] of Johns Hopkins University, went to Nancy and spent three hours in the laboratory looking upon experiments with the N rays without seeing a single experiment succeed. He departed in the belief that those who had seen them had been deluded. It was arranged to have an observer tell when Prof. Wood put his hand in the path of the rays by noticing accompanying changes in a small spark, but in no case was the statement correct.

In the experiment of the spectrum produced by an aluminium prism, a beam 2 or 3 mm. wide was said to give after dispersion a band of about .1 mm. width. When the prism was covertly removed, the bands were located as before. Prof. Wood suggested that he place the prism while the N ray expert determine how it was turned. Incorrect answers were given in all of the three trials made. This failure was attributed to fatigue.

A steel file was brought up to the screen, but Prof. Wood could see no effect. He substituted a piece of wood for the file and others saw the changes as before. He suggests a repetition of the Pender-Cremieu collaboration as a good way of reaching the truth about the N rays.

Criticisms which he made upon the technique of photographing the spark under N ray influence will be considered elsewhere.

Blondlot[52] replied to Wood's criticism November 12th. He said Wood was warned that a person observing a phosphorescent screen could not necessarily tell when another person threw the N rays on the screen. "I affirm most positively," he says, "that the phenomena of N rays have for me the same certainty that other physical phenomena have. Several of my colleagues and a number of other persons say the same." He adds explicit directions for observing the N rays with the screen.

In October[205] the Revue Scientifique began an editorial discus-

sion of the status of the N rays. "Depuis le début des vacances, on ne parle plus en France de rayons N. Mais à l' étranger on continue à accumuler les expériences infructueuses, les resultats négatifs. Le doute ne cesse de croître."

"Il n' y a pas un pays étranger où les rayons N aient encore le moindre crédit et en France même on ne songe guère a discuter sur l' emission pesante * * * ni même sur la production de rayons par l' organisme."[206]

This periodical then obtained expressions of opinion upon the subject from many French scientists.[207,208,209,210,211,212,213,214]

The following believe in the N rays,—Bertholet, Pellat, Blondlot, Janet, Gariel, Lambert, H. Poincaré, D'Arsonval, Moreau, Swyngedauw, Girardet, H. Becquerel, Colson, Bichat, Gutton. These do not believe,—Langevin, Perrin, Monoyer, Brunhes, Lamotte, Cailletet, P. Weiss. The following failed to get results,—Pellat, Abraham, Perrin, Janet, Sagnac, Cailletet, Berget, Gouy, Monoyer, Meslin, Buisson, Camichel, Turpain, Weiss (Paris), Doumet. It will be noticed that in spite of this failure some of them believed in the existence of the disputed rays.

The attitude of Mascart is worthy of note. He expressed no opinion about the N rays, though Blondlot certainly believed that he, as well as Cailletet, was convinced, as is shown in a letter to the London Electrician.[51]

D'Arsonval also states that both Mascart and himself could distinguish effects upon the phosphorescent screen. More than a year later Mascart[123] appeared as the champion of the N rays, when nearly every one else had lost all belief in them.

Others who expressed no opinion are Bouty, Violle, Brillouin, Leduc, Moissan, P. Curie, Chappuis, Moissan.

Perrin, professor of chemical physics in the Sorbonne, is very emphatic in his opinion: "I think and repeat on every occasion for more than a year not only that there are no N rays, but that there is no objective phenomena capable of justifying the strange error of those physicists who have seen the rays."

Pellat, who expressed his belief in the existence of the rays, though he failed himself to get results with them, held that few can see them, and that the fortunate are rather the young and normal.

Cailletet says that he saw no results during his visit to Blond-

lot at Nancy, though there were many ladies present who saw the effects well.

Meslin went to Macé de Lepinay for assistance but did not feel much confidence in his experiments.

Buisson, assistant professor of physics at Marseilles, also saw little when the Marseilles investigator endeavored to show him the effects.

H. Poincaré was at Nancy but saw no effects that he was expected to see. This he attributes to involuntary accommodation of the eyes on his part. He believed in the reality of the photographic effect.

D'Arsonval stated that he had verified all the important facts, perceiving clearly the action of the nervous centers on the phosphorescent screen.

Weiss, Professor of Physics in the Faculty of Medicine, Paris, claims that Charpentier's determinations of the length of nerve-waves are wrong, and that this shows his N ray results to have a subjective element, because by his N ray method, he obtained about the same result as by his other incorrect method.

Colson stated that Blondlot and Wirtz saw just what he did in the case of the production of the rays by chemical means.

Very interesting is the statement of P. Weiss, Zurich. It was he who arose at the Breslau meeting and in face of the general sentiment against the existence of the N rays, stated that his experiments, though incomplete, seemed confirmatory. Now in the Revue Scientifique he reported that he was mistaken and that his results are negative.

L. Poincaré, inspector general of public instruction, could not see Blondlot's results.

The French journal Cosmos[237] speaks of the varying views about Blondlot's rays: "Ce mouvement d' incrédulité est parti d' Amérique, nous est arrivé par l'Angleterre et a gagné le Continent."

In spite of the bombardment of criticism during the latter half of 1904, there were a few who still continued to hold to the reality of the N rays.

Hooker[113] wrote another letter to the Lancet: "I take it that there are but few who do not acknowledge the existence of the 'n' rays." He presents an experiment to show that the results cannot be due to heat. The value of his observations is much

lessened by his subsequent statement that he carefully examined the "luminous ray spectrum, finding *inter alia* that rays emanating from a passionate man have a deep red hue; the one whose key-note in life is to be good and to do good throws off pink rays."

There is also another letter in the Lancet written by Niven[134b] expressing belief in the N rays.

Colson[106] continued his chemical investigations suggested by N ray effects previously obtained by him. A little more than a year later the Academy of Science bestowed upon him the La Caze prize for his general work in chemistry.

Breydel[60] advanced the theory that the N_1 rays are a vibrating impulse emanating from their source, while the N rays are a kind of movement of concentration converging toward their source. He also postulates the existence of another type of rays, the N_2 rays.

In the month of November Blondlot[48] published another paper relating to the photography of the spark when acted upon by N rays. This will be discussed later.

In December, 1904, the Leconte prize of 50,000 francs was awarded to Blondlot[236] by a committee of the Academy of Sciences consisting of Mascart, Troost, Darboux, Berthelot, M. Levy, H. Becquerel, Bouchard, Moissan, Janssen, de Lapparent and Poincaré. In their recital of Blondlot's scientific work but scant reference is made to his investigations of the N rays.

Before the year 1904 reached its close Bordier[59] presented in the Comptes Rendus a photographic method of showing the effect of the N rays on a phosphorescent screen. Becquerel[8] had contended that the rays produce no effect upon the brightness of the screen, a view with which Blondlot[42] subsequently agreed.

Bordier prepared two similar groups of patches of Ca S and exposed them to the light for some time. Each group was put upon a photographic plate. Upon one was laid a file to serve as a source of N rays. Upon the other was laid a piece of lead of equal weight. After twenty-four hours in the dark the plates were developed. Each patch of the phosphorescent substance was found to be surrounded by an aureole. The aureoles about the patches exposed to N rays were more extended than the others. A man, who was not acquainted with the design of

the experiment, measured the central images and the aureoles in both sets. His measurements showed a difference.

A fortnight after the publication of this paper the Revue Scientifique[215] states that the claim that the aureoles were larger about the patches exposed to N rays was not confirmed when a number of the members of the Institute examined the plates, and directs attention to the possibility of electrical phenomena, occurring in the course of the experiment, leaving traces on the sensitive plate. Besides this Le Bon has shown that the photographic plate is unequally acted on by different metals.

Weiss and Bull[192] reported a fruitless search for a direct effect of the N rays upon a photographic plate. A sheet of paper uniformly illuminated was photographed. N rays fell upon the plate at two places. On development these showed no difference from the rest of the plate.

Apropos of Bordier's experiment, Chanoz and Perrigot[150] reported early in January, 1905, the results of their examination into the conditions necessary for the production of aureoles on the photographic plate by Ca S. They found

1. A very brief time only is needed to produce aureoles.
2. The size of the aureole varies with the thickness of the layer of Ca S.
3. The size of the aureole depends upon the distance of the Ca S from the plate.
4. When two identical screens of Ca S are put under equal weights at the same instant on the photographic plate the aureoles are the same no matter of what material the weights are made.

During 1904 Kotnik[115] and also Bechterew[6] suggested that N rays may explain thought transferrence. According to the former the language centers emit N rays which excite the corresponding centers of the person on whom they fall and evoke auditory images.

Since the beginning of 1905 it is only after long intervals that experimental papers dealing with the Blondlot rays appear. The last paper by the discoverer of the N rays appeared in August, 1905.[53] In connection with this it may be well to take a general view of the work done in registering photographically the effect of the N rays upon a small electric spark.

As early as May, 1903,[28] Blondlot reported the existence of a difference between the images produced upon photographic plates

by the light of the spark when N rays fell upon it and when they did not. In Feb., 1904,[38] he furnished details of a more accurate method. A stream of small electric sparks illuminates from above a horizontal ground glass plate. Below this and parallel to it is exposed a photographic plate. A cardboard box having the plate for its bottom covers both the spark and the glass plate. Above this box is a lead plate covered with wet paper. This plate is fixed in position with respect to the photographic plate, and thus both must be shifted together. A beam of N rays comes vertically downwards. When the screen of lead is in their path the uninfluenced spark casts its image on the ground glass and from this an image is produced on the photographic plate. Next the lead screen is shifted from the path of the rays and with it goes the portion of the sensitive plate already acted on. A fresh part of the plate is now under the ground glass which is illuminated by the spark upon which the N rays now fall. In carrying out the experiment the alternate arrangements lasted for five seconds. The lead plate was shifted to and fro several times, the times of exposure of both halves of the plate being the same. By means of guides the shifting was performed in darkness. Much care was taken to get the spark feeble as well as regular. About forty experiments were reported, using as sources of the N rays a Nernst lamp, compressed wood, tempered steel, etc. In but a single case were the images on the two parts of the plate alike.

A few months later[44] Blondlot describes certain improvements in his method, such as concentrating the N rays by an aluminium lens and giving regular form to the terminals between which the spark passes. A few days after this Burke[148] reported his failure to find a difference between the two parts of the plate, and suggested that the interposition of the lead screen to intercept the N rays might damp the spark.

Lummer stated at the meetings of the British Association for the Advancement of Science[197] and of the *Versammlung Deutscher Naturforscher und Aerzte*[196] that he himself and also Rubens had been unable to obtain this photographic effect. They were favored by Blondlot with some of his own plates, but failure continued to meet them.

Wood[193] estimated that at the time of his visit to Blondlot's laboratory the brilliancy of the spark fluctuated by as much as

25 per cent. In his opinion the unconscious bias of the person shifting the plate might give the advantage of longer time to the part of the plate exposed during the period of action of the N rays on the spark. In his reply to Wood's criticism, Blondlot[52] says that since the latter's visit he has used an apparatus which recorded automatically the times of exposure. In a total exposure of each side amounting to about fifty seconds, the difference between the two periods of exposure was not more than .5 seconds, and moreover the benefit of the longer exposure was given to that part of the photographic plate used when the N rays were shut off. In spite of this, in every one of twelve experiments there was a larger image obtained when the rays had access to the spark.

Blondlot[48] in a paper appearing in November, 1904, turns the flank of Burke's suggestion that the moving of the lead screen affected the spark by stating that he had performed the experiment with no N rays and had found no difference between the two images. The spark-gap was surrounded by a conductor joined to earth but the difference in the images persisted.

In Blondlot's latest paper[53] he adduces a series of thirty-five experiments in support of his contention. In twenty-three of these there was a very marked difference between the two images, and in all a difference was visible. He claims that the application of improved methods have only made the obtaining of the effect more certain. He further says that more than 100 photographic experiments have been made by Jean Becquerel with constant success.

Even if it be granted that a new and objective effect has thus been obtained, let it be noted that it in no way argues for the existence of the N rays with their varied and surprising properties. All the evidence for their kinship to light waves was obtained by subjective methods, chiefly by the use of the calcium sulphide screen.

In the course of the year 1905 four experimental investigations told against the Blondlot rays. Guilleminot[158] permitted N rays from the Nernst lamp and from tempered steel to fall upon a selenium cell, either alone or accompanied by light, but in neither case could he detect any change in the resistance of the cell due to the rays.

Pozdena[179] made a very painstaking investigation of the

effects claimed for the heavy emission. His paper is a model of accurate work. In contrast with many of the French papers the greatest care was exercised to eliminate all suggestion and other subjective elements.

A silver gulden was the source of the alleged emission. One person moved the Ca S screen about in the space below the coin, another observer stated when the screen appeared brighter. A piece of lead could close an aperture below the coin thus cutting off any emission. There was total darkness throughout. Slight changes of sound made in moving the screen were avoided. When one position had been located in which the screen seemed brighter, it was noticed that the observer had a recollection of the position of his head and of the direction of sight. This tended to lead him to locate the position of brightness again where it was before. Moreover if the person in charge of the screen moved it rather more frequently over one spot a hint was given to the observer. To avoid these sources of suggestion the two persons changed positions after a spot of brightness had been found.

It was further noticed that the observer could tell by the sound whether the other wrote "o" for *offen* or "g" for *geshlossen* in recording whether the lead allowed the emission to pass or not.

Pozdena found no evidence whatever of the existence of a heavy emission. Only three times out of 150 settings did the phosphorescent screen come vertically under the coin, and in one of these few cases the lead was interposed.

In sixty-eight cases when the lead intervened the brightening of the screen was noticed as well as in eighty-two cases without the lead.

Stefanelli[185] found that the lessening of the brightness of the phosphorescent screen under the bell-jar of an air pump, which is caused by exhaustion, is really due, not to the emission of N_1 rays as J. Meyer[129] supposed, but to the fall of temperature of the enclosed air. Bellia[145] failed to get photographic effects.

The number for Jan. 15, 1906, of the Comptes Rendus contains two papers in support of the existence of the N rays. Mascart[123] describes a series of experiments in which a narrow Ca S screen mounted on the carrier of a dividing engine was moved through the space behind an aluminium prism which was intended to disperse the N rays derived from a Nernst lamp. The carrier

was moved until the screen lighted up presumably because it was in the path of one of the refracted rays. The corresponding reading of the position of the carrier was made. The screen was moved through the same extent of field by each of the four observers who in turn located the position of brightness. The following gives the readings of the settings for four positions of brightness:

Blondlot	Gutton	Virtz	Mascart
382.4	Not observed	381.	383.4
Not observed	387.2	386.9	387.
391.5	393.	392.	391.
398.4	399.	398.2	397.

Another series of settings was made by each observer in which the carrier was moved forward so as to set the screen in turn upon each bright band, then the motion was reversed and the settings were made as the carrier came back.

The series which follows was made by Blondlot:

Going	387.5	382.3	374.	368.2	360.2	358.	353.2
Returning	386.1	381.2	374.3	368.2	360.2	358.2	353.2

Gutton was not so successful in obtaining concordant settings. In one case the two readings for the same band differ by 4, when the distance between two adjacent bands is only 6 or 7.

But few details of this experiments are given in the paper.

The Revue Scientifique[216] subjects the above experiments to criticism. It directs attention to the fact that the readings of Virtz and of Mascart for the first band differ by 2.4, and in view of the entire field examined being only 16 in length, it concludes that "The precision is not really excessive."

In the experiments made by setting on the bands of brightness both going and returning it found that the space in which no bands were found was considerably less than the space in which bands were located.

The periodical suggests that as Blondlot made the experiments first the other experimenters may have unconsciously copied him. "Could not muscular memory account for the agreement of the settings made going and returning?"

Moreover, Blondlot had agreed with Becquerel that the apparent effect of the N rays upon the phosphorescent screen is in fact due to the rays producing an effect upon the retina. In spite of

this, how could Blondlot be consistent in taking part in the experiments described by Mascart?

Perrin[177] directs objections against the experiment from theoretical considerations.

Turpain[190] took up the going and returning experiments. He found that if he gave 10 impulses to the driving mechanism of the carrier of the dividing engine, then took the reading, gave 10 more impulses, took the reading, and so proceeded, he obtained the following series:

Going	4.58	9.65	14.26	18.58
Returning	4.54	9.61	14.95	18.51

There was no setting upon anything. The agreement follows from there having been the same number of equal impulsions.

Turpain seems to have made his investigations very carefully. He says: "For more than a year, except for short interruptions, I have pursued the observation of the N rays." His results while using the Ca S screen were concordant so long as he knew whether the rays were present or not, but became discordant when he was in ignorance. He often found a suppression of the N rays to lead not to a decrease but to an increase of brightness.

Experimenting with the Nernst lamp, files, the electric field, the magnetic field, the electromagnetic field, the Hertzian field, out of 962 trials the presence of the rays was detected in 783 cases, or 81%, when he knew what to expect. On the other hand the percentage fell to about 50 when he was not informed.

An interesting incident connected with Turpain's paper was its rejection by the Academy of Sciences.[220]

(*To be concluded.*)

(Stated Meeting held Thursday, May 2d, 1906.)

A Recent Development in the Chemistry of Cellulose.

By Dr. William H. Walker,

Professor of Industrial Chemistry, Massachusetts Institute of Technology, Boston.

Of the numberless bodies which now make up that department of chemical science which treats of the compounds of carbon comparatively few belong to the class to the consideration of which was originally applied the name "organic chemistry." In the early days of Wöhler, only those substances which occur organized in nature and which are formed in the animal and vegetable kingdoms only under the influence of a special, obscure agency called the vital force, were included in this classification. Among the few bodies which have hitherto resisted synthesis and which are as yet made only through the intervention of a vital force, are those which form the skeleton or framework of all vegetable tissues, and which are included under the generic term "cellulose." All plants, from the microscopic unicellular bacterium up to the giant conifers of the Yosemite Valley, are built up of cells, the envelopes or walls of which consist essentially of this organic compound, cellulose. From the earliest times, cellulose in its various forms has constituted the most important raw material used by man, either in its substantially pure state, as cotton or linen, or where it is associated with other more or less complex bodies in wood, bamboo, straw and other structures built up by the plant. For many years the commercial value of cellulose depended entirely upon its physical properties and the form in which it was obtained. When it became possible to modify the natural properties of this material by chemical treatment, its enormous economic importance was still further increased and its commercial value enhanced.

While the word "cellulose" may be used to include bodies of analogous composition having similar functions, it must not be

taken as signifying a simple, definite substance of unvarying properties. Just as we have wheat starch, potato starch and corn starch, varying in size and shape of granule, in viscosity and adhesiveness of the product made therefrom, etc., so, too, we have different forms of cellulose. Physically, these substances are, when freed from adhering waxes, resins and other incrusting materials, colorless, amorphous bodies capable of withstanding relatively high temperatures and soluble in none of the ordinary solvents. An ultimate analysis reveals the fact that cellulose is composed of carbon, hydrogen and oxygen in the proportions represented by the empirical formula $C_6H_{10}O_5$. This statement in no way, however, represents the true molecular complexity of the substance. Starch is represented by the same molecular proportion $C_6H_{10}O_5$. Superimposed upon what is probably a very complicated molecule in both of these substances, is what we call the colloidal state, a condition of matter of which we at present know almost nothing. In contradistinction to the sugars, the third closely allied member of the so-called carbohydrate group, we have in cellulose no clearly defined chemical or physical constants, such as melting point, boiling point, solubility relations, etc., to guide us. Although, as we shall find, the chemical reagents which act upon cellulose are not few, this action, unfortunately, is always accompanied by a more or less complete breaking down or splitting of the so-called cellulose aggregate. Through a study of those derivatives of cellulose and starch known as the bioses, where two $C_6H_{10}O_5$ groups combine with one molecule of water, Skraup, by means of a chlorine-acetyl compound, arrives at the conclusion that the simplest possible molecular weight of cellulose is 5508, while starch is 7440. This much, at least, is certain—that the forms of cellulose of which we have knowledge, are combinations of a large number of groups having the three elements in the proportion expressed by the formula $C_6H_{10}O_5$, and that, when undergoing chemical reaction, the more perfectly this complex or aggregate can be kept intact, the more valuable from a commercial point of view, are the resultant products.

A reaction which is common to all three members of the group of compounds, known as carbohydrates, viz., sugars, starches and cellulose, but which these substances show to a very unequal degree, is that of hydrolysis by dilute mineral acids. The sugars and starches are converted into bodies having an increased

percentage of hydrogen and oxygen, with the greatest ease, forming dextrose and levulose in the first case, and in the second a mixture of dextrose and other sugars. The enzyme diastase is also able to effect transformations of this character very easily. When we come to cellulose, however, we find a marked difference in the ease with which analogous reactions are brought about. Treatments which effect complete hydrolysis in the case of starch, has little or no effect upon cellulose. A different set of conditions are necessary to break up the cellulose aggregate and to add on the water necessary to form these simpler molecules or molecular complexes.

The earliest worker in this field to carry on systematic investigation and to obtain tangible results, seems to have been a Frenchman, Girard,* in 1881. By subjecting cellulose to the action of dilute sulphuric acid for several hours he obtained an easily pulverized mass, which he called cellulose hydrate and which he thought had the formula $(C_6H_{10}O_5)_2 . H_2O$. A few years ago Stern† repeated this work and failed to get the results described by Girard. He found, however, that if cellulose be boiled with 5% sulphuric acid, a part is converted into dextrose while the major portion is simply disintegrated. This disintegration is due to the fact that certain portions of the cellulose fibre are more easily attacked than others, and as these portions are dissolved their removal causes the structure as a whole to fall to pieces. Since Stearn did not follow, in any case, the directions of Girard, it is not surprising that comparable results were not obtained. The fact remains that the product of such acid treatment shows physical properties not possessed by the original cellulose.

This hydrolysis may be carried to the point where practically a complete conversion of the cellulose into soluble and for a large part also fermentable sugars, takes place. On this fact is based the much-talked-of process for obtaining alcohol from wood, of Classen and others, where dextrose is first formed and then subsequently fermented.

A relatively large number of processes for making hydrocellulose, differing in detail from the above, have been proposed, but they all result in a very friable powder in which the original

*Ann. de Chem. et Phys., *24*, 350.
†Jour. Chem. Soc., *85*, 336.

structural form of the cellulose is lost. They all seem to bear out the general formula $(C_6H_{10}O_5)n.\ H_2O$.

One of the more common methods used by investigators in the field of organic chemistry for determining the number of hydroxyl groups in a compound, is to substitute each one by an acetic acid radical, making a so-called ester. This esterification is usually effected by treating the hydroxyl-containing substance with acetyl chloride or acetic anhydride, with or without the presence of some condensing or esterifying agent, such as sodium acetate, zinc chloride or sulphuric acid. By separating the products of this reaction and then subjecting them to treatment with alcoholic potash, the acetyl groups may be split off and measured, and the number of hydroxyl groups determined.

A. N. P. Franchimont,* in 1882, while studying the problem of the constitution of cellulose, used this method. He found, however, that if the cellulose be treated with acetic anhydride without a condensing agent, the conditions necessery for the reaction were such that an almost complete disintegration of the cellulose aggregate resulted. However, by adding sulphuric acid to the reaction mixture, a hydrocellulose was formed and the esterification then took place with great ease. He points out that esters differing widely in solubility can be produced by varying the proportion of sulphuric acid. It is thus seen that Franchimont was the pioneer in this field of cellulose chemistry and opened up the way for the great amount of work which has recently been done upon this subject.

Lederer,** in his method for the production of the acetic acid esters of cellulose, provides for making hydrocellulose according to the method of Girard, and subsequently esterifying by acetic acid in the presence of a small quantity of sulphuric acid. Miles,† repeating the method of Franchimont, places the sulphuric acid in the reacting mixture and makes the hydrocellulose at the same time that the acetylation is effected. H. S. Mork,‡ working in conjunction with A. D. Little and the writer, found that by using the sulphonic acids of phenol or naphthol, the reaction is more

*Rec. trav. chim. Pays-Bas., *18*, 472.
**U. S. Patent, 654,988.
†U. S. Patent, 733,729.
‡U. S. Patent, 709,922.

easily controlled and a more uniform product obtained. Other investigators have proposed phosphoric acid, methyl sulphate and a variety of other hydrolyzing and condensing agents, without obtaining a materially different product. In all these cases, as in the original method of Franchimont, the resultant ester is dissolved by the reaction mixture and is recovered by precipitation therefrom in a large volume of water. The product of this procedure has been described as tetracetate of cellulose, and its appearance depends upon the way it is precipitated by water from its solution in acid. When saponified in the regular way with alcoholic potash, the product gives values corresponding to the tetracetate, although the yield corresponds to that of the tri-acetate calculated on the $C_6H_{10}O_5$ basis. Thus, 100 grams of carefully prepared cotton roving yields when acetylated with benzol-sulphonic acid, 168 grams of product, while that calculated for the tri-acetate is 177 grams and of the tetracetate considerably more. The saponification number is found to be about 705, while that calculated for the tri-acetate is 583 and that for the tetracetate 700.

Much light has been thrown upon this apparent discrepancy by a recent paper by Dr. H. Ost,* in which the analysis of these acetyl derivatives is discussed at length. Data are given which show that when the esters described above are saponified in the ordinary manner with alcoholic potash solution, acids other than acetic are formed, which, united with the potash, give an abnormally high saponification number. If the products of this saponifying action be mixed with sulphuric acid and the acetic acid distilled with steam, an amount of acid agreeing closely with that calculated for the tri-acetate is obtained. A method proposed by H. E. Perkin for determining the acetic groups in phenols was found by Ost to be especially applicable to the analysis of these products. It consists in gently heating the ester with 50% sulphuric acid solution for twenty-four hours or more, and then separating the acetic acid thus set free by distillation with steam. The figures obtained agree very closely with those calculated for cellulose tri-acetate, notwithstanding the fact that the products investigated were made by a number of different processes. The so-called cellulose tetracetate of Cross and Bevan, made by treating the cellulose recovered from the sulpho-carbonate reaction with

*Zeitschr. f. Angewandte Chem., *19*—993.

magnesium acetate and acetyl chloride, was examined by this method by Perkin and Greene and found to be in reality the tri-acetate. Of course, it is possible that these products consist of a mixture of di- and tetracetates in such a proportion that the saponification equivalent is that of the tri-acetate, but while there are indications that di-acetates do exist in the mixture, the above supposition is quite improbable.

The fact noted by Franchimont that acetates of very different solubilities could be obtained by varying the amount of sulphuric acid employed in the reaction also lead to the development of a number of new cellulose derivatives.

There are unquestionably two separate reactions taking place simultaneously, the first being the production of the hydrocellulose by means of the acid, and the second the formation of the ester of this hydrocellulose by the acetic anhydride, accelerated by the presence of another acid or its equivalent. Taking up first the preparation of hydrocellulose, we find that the method proposed by Girard was to soak the well purified cellulose in a 3% sulphuric acid solution until saturated, press tightly between filterpapers and dry in the air. The paper thus treated was heated in a closed flask to 70° for three hours. The resultant product was a brittle, white mass which reduced Fehling solution slightly and was more deeply colored with fuchsin solution than before the treatment. Mork, in repeating this work, found the method very difficult to control, and very sensitive as regards the pressure on the cotton before drying, and the temperature and time of heating. By using too much acid or heating too long, a product is obtained which is probably the highly degraded cellubiose of Skraup $(C_6H_{10}O_5)_2 . H_2O$. Lederer has varied the method of Girard by substituting acetic acid for water, but obtained the same brittle mass, which soon fell to a structureless powder.

A very much more valuable product of uniform quality, as shown by Mork, can be prepared as follows: Into a mixture of 400 parts glacial acetate acid and 20 parts benzol-sulphonic acid are placed 100 parts of high-grade bleached cotton roving. This is allowed to remain until perfectly saturated with the liquid, when the mass is pressed until one-half of the liquid originally used has been recovered. The wet cotton, now holding 200 parts acetic acid and 10 parts benzol-sulphonic acid, is allowed to

stand at room temperature (15° to 20° C.) for about twelve hours. At the end of this period it will be found to have been converted into a highly reactive modification or form of cellulose which retains the original fibrous structure of the organic cellulose. When washed and dried the product is found to have increased in weight about 1.88% on the weight of the cotton used. This corresponds to the formula $(C_6H_{10}O_5)_6 . H_2O$, which would require an increase in weight of 1.85%. This process admits of perfect control and produces a hydrocellulose in which the original cellulose has suffered little, if any degradation.

In the recent publication already referred to, Ost confirms these results, although he gives as the optimum conditions a somewhat greater proportion of acid to the cotton, and allows the mixture to stand for two days. From data obtained by the ultimate analysis of his product, Ost arrives, quite independently of Mork, at the same empirical formula $(C_6H_{10}O_5)_6 . H_2O$. He confirms Mork's observations also, that too energetic treatment leads to a disintegration of the cellulose aggregate, forming products of much lower molecular weight containing a relatively larger quantity of water of hydrolysis.

All of these hydrocelluloses in the presence of the condensing agent react with ease with acetic anhydride, giving results which depend upon the extent of the hydration, the temperature of reaction and the time. Products may be made which are soluble in water alone, alcohol and water, acetone and water, acetone alone and chloroform alone. In obtaining the results given in the following table, cotton was treated with the hydrolyzing agent and acetic acid for the time given in the first column, and, without washing, was acted upon with acetic anhydride. The relative solubilities appear in the other columns, where *i* means insoluble, *ps* partially soluble, *g* gelatinized, and *s* soluble.

Cross and Bevan* have investigated an analogous series of substances and conclude that these compounds are the mixed esters of sulphuric acid and acetic acid. When a large amount of sulphuric acid calculated on the weight of the cotton was used, a material which was soluble in water was obtained, which contained the proportion of one sulphuric acid group for each $C_6H_{10}O_5$ group. By decreasing the amount of sulphuric acid the

*Ber., *38*, 38 and 1859.

% Benzene Sulphonic Acid (on cellulose)	Time (Preliminary)	Temperature of Acetylation	Time of Acetylation	Solubility				
				Water	Ethyl Alcohol and Water	Acetone	Acetone and Water	Chloroform
1%—2%	None	80°—100°C.	1—2 hrs.			i.		s.
10%	24 hrs.	20°C.	24 hrs. 72 hrs.			i. i.	s. s.	p. s. s.
25%	24 hrs.	20°C.	24 hrs. 72 hrs. 96 hrs. 168 hrs.			p. s. p. s. p. s. p. s.	s. p. s.	i. p. s. s. s.
50%	24 hrs.	10°—15°C.	2 hrs. 15 hrs. 48 hrs.	s. g. i.		s.		i.
50%	24 hrs.	5°—10°C.	96 hrs.		s.	s.	s.	i.

product was no longer soluble in water but was soluble in aqueous acetone and contained less sulphuric acid and more acetic acid groups. When a relatively small amount of sulphuric acid was present, the more normal acetone- and chloroform-soluble acetates were obtained. So far as I know, the analytical results given by the authors in support of these statements have not been confirmed by others, but the fact that by increasing the amount of sulphuric acid, products of radically different solubility may be obtained, confirms the work of Franchimont and Mork.

The technically valuable acetic acid ester of cellulose, however, is the tri-acetate, although as generally made, some di-acetate is present. The tri-acetate is easily soluble in chloroform while the di-acetate is much less so, being scarcely gelatinized. The increasing solubility in chloroform, due to the change of di- into the tri-acetate as the reaction proceeds, is shown in the following experiments:

Hydrocellulose made as above described was treated with acetic anhydride at 40° C., giving the following data:

Time	Solubility in Reacting Acids.	Solubility in Chloroform.
30 min.	Soluble	Insoluble
1½ hr.	"	Slightly soluble
4½ hr.	"	Very soluble

It will be noted that all of the acetic acid esters of cellulose, in contradistinction to the nitric acid esters, have been heretofore produced by methods which have involved, as a necessary consequence of their successful application, the complete breakdown and destruction of the physical form of the cellulose structure concerned in the reaction; that is, the product of the reaction is soluble in the reacting mixture. The great practical disadvantages incident to this simultaneous solution will be apparent. Among them may be mentioned the difficulty of control; the danger of local overheating which makes necessary the working of small batches; the necessity of precipitation in water to check the reaction; and the dilution, and to a considerable extent loss, of the reagents employed. It has been the goal of investigators in this field to produce an acetic acid ester of cellulose which will retain the structural form of the original cellulose in a way analogous to that of cellulose nitrate. This has been successfully done by H. S. Mork in the following manner:

100 parts hydrocellulose is prepared as has already been out-

lined, and while still moist with the hydrolyzing acid, is placed in a mixture of 300 parts acetic anhydride and 1200 parts benzol and allowed to stand at room temperature (15° to 20° C.) for about eighteen hours. When the reaction is complete, as ascertained by removing some of the product and determining its solubility in chloroform, acetone, or other appropriate solvents for the particular ester toward the manufacture of which the process has been directed, the mass is freed as far as possible from liquid, by whizzing in a hydro-extractor, and is then washed and dried. Among the important advantages offered by this process may be enumerated the complete control which results from the moderation of the rate of esterification by reason of the low temperature at which the same is conducted, coupled with the influence of the benzol, or its equivalent, in restraining the reaction; the ease of testing the progress of the process and of stopping it at any time desired; the large quantities of material which may be handled without endangering the quality of the product, and finally the ease of washing and drying the product in bulk.

The analysis of this product shows it to be the tri-acetate of cellulose, but differing from an analogous fibrous acetate which has been prepared by Strehlenert and Reubold,* in that it has apparently preserved the large molecular aggregate of the original cellulose. The material itself shows greater strength than the acetate of Strehlenert (which is described as being brittle); gives more viscous solutions than acetates hitherto prepared, and is so far as can be determined a superior product in all its properties.

The uses to which cellulose acetate may be put are at present limited by the expense of the material itself, incident to the cost of the necessary reagents and the relatively high price of its solvents. In general, it may be said that cellulose acetate can find technical application most readily where nitrocellulose, on account of its inflammability and low temperature of decomposition, cannot be employed. At present its most important use is in the electrical insulation of very fine wire, for which it has found an extended application in the hands of the General Electric Company at Lynn, Mass. A highly desirable quality of artificial silk and artificial horsehair can be made from the acetate, but for the present it is ruled out of the market by its cost of production.

*U. S. Patent, No. 812,098.

Mining and Metallurgical Section.

(Stated Meeting held Thursday, May 9th, 1907.)

Black Sands of the Pacific Coast.

By Dr. David T. Day.

When any new country enters the list of civilized nations, its mineral deposits are greatly sought and it begins the age of bounteous mineral yield and consequent profligate waste. It is common to use only the richest deposits, and, of these, frequently only the richest parts.

It is a case of sudden access to riches with the usual squandering which has no regard for economy and no care for posterity's interests.

In the case of the United States we were slow to recognize our supremacy in mineral wealth over the rest of the civilized world.

For the first three hundred years and more we merely harvested the obvious gold in the placers of the Southern States and satisfied our needs for household utensils from the iron of Pennsylvania and New England.

The California gold rush of 1849 and '50, however, changed our mineral industry for all time and made keen the search for hidden treasure, everywhere. It brought out the ignorant fortune seekers who cared more for unearthing something hidden than for the development of legitimate industry. This greed of the ignorant has pursued precious metal mining to the detriment of the industry to the present day.

When in 1859 the development of petroleum in Pennsylvania furnished a flood of light to turn night into day we became a nation of *readers*. Before that none but the clergy read at night. Our great industrial advance was due fundamentally to the fact that nowhere in the world did it cost so little to read at night as in the United States, and this is still true.

With the resulting flood of information, the mineral requirements for great industrial developments exceeded all anticipation. Discovery of new and rich mineral deposits kept up with the demand. The State and National Geological Surveys taught people where to find new deposits and how to mine them with speed rather than economy. Our development increased so fast that with coal (for example), the production of any ten years has equalled the total production of all previous years since the industry began.

Of course no country's mineral resources are so boundless as to stand this depletion for any great length of time. Already the days of the anthracite coal trade have been numbered by the statisticians, and experts from France and Germany have been sent by their Governments to predict the end of our Lake copper and iron ores.

While these experts have returned obliged to confess the greatness of these deposits, it is significant that the trade this year in seeking a greater output of iron ore from the Lakes is not so exacting as to quality. In Pennsylvania the days of anthracite are being lengthened by washing the valuable coal from the old culm banks and by increasing the percentage extracted by every practicable means.

The day of gleaning only from our richest treasures is *past*, and already we are turning to deposits heretofore neglected because of impurities rendering them hard to work, or because of their low grade.

The concentration of low grade ores of all kinds is fast becoming an important feature of the mining industry.

Ores of precious metals have naturally received first consideration, and it is nothing extraordinary to see a hundred mechanical concentrating tables washing the ore of a single set of mines.

In older countries such economies are more common. But in spite of this the supply of one metal, platinum, has become alarmingly small. We are accustomed to import it from Russia, but the yield of the placer deposits in the Urals has declined sharply in the last few years, and pressure was brought upon Congress to have the Geological Survey search for it in the United States. We had already studied out its haunts and knew that it was not a problem of seeking new sources of

supply, but rather a matter of finding means of gleaning it cheaply from the waste of certain placer gold mines,—placers which came from certain great ridges of serpentine in California and Oregon, in which grains of platinum have been found here and there, but too sparsely to afford profitable mining in the original rock. It was necessary to seek it where the decay of such rock had left the platinum and other heavy materials richer by the washing away of the lighter ingredients.

The Geological Survey (which had pointed out the conditions for finding and utilizing our rich stores of wealth) was thus called to teach the art of saving by-products to men who were accustomed to staking everything in the chance of finding hidden treasure and who had thrown away, with contempt, everything but gold.

First it was necessary to analyse these sands and ascertain what they contained of value.

The definition of what is a useful mineral depends upon the times. Trade to-day demands various rare minerals which were merely cabinet curios a few years ago. There is nothing in those sands with the exception of platinum which is not already obtainable somewhere else, and the question of making these minerals useful depends upon the cost of separating them from valueless constituents.

First, a word as to the source of black sands.

When a placer miner washes down a gravel bank, all of the sand heavier than ordinary quartz collects in the bottom of his sluice box, together with the gold which he is trying to save. These heavy minerals are collectively known as "black sands." Perhaps the miner only "sluices" his gravel without the help of a hydraulic giant.

As soon as sufficient "black sand" collects in the sluice box to fill the riffles level full, these riffles no longer catch the gold and the operation must be stopped until the riffles are cleaned out, the gold panned out and the "black sand" thrown away. If the sands contain from thirty to fifty per cent. of "black sand," as is sometimes the case, the suspension of mining in order to clean out the riffles is necessary so frequently as to render the mining profitless, even when the sand is quite rich in gold. By this means small quantities of "black sand" are

continually being thrown away, and yet in the aggregate the amount is large.

The sands of the Pacific seashore have been washed so free from light sand by the action of the waves that practically all of the seashore sand is known as "black sand," and has accordingly a bad reputation, especially as the gold in these seashore sands is found in proverbially fine-grained sand, frequently as fine as ordinary flour. Further, the gold grains in the "black sands" are frequently so coated as to give difficulty in amalgamating and therefore it is particularly difficult to extract this seashore gold.

It has been the habit for many years in the West to throw away thousands of tons annually of these black sands without any effort even to determine the proportion of gold and platinum which they contain, because, even when quite rich, no practical method of obtaining the gold has been available. Hundreds of processes have been attempted for this purpose and abandoned.

When the Geological Survey was appealed to to study the principles governing the occurrence of platinum in the United States, we first collected some 2,000 samples of these "black sands" by inviting the placer miners of the United States to send in for examination samples of the heavy minerals found in their sluice boxes. The results of these investigations were of unusual interest. Among some thirty minerals found in these sands, the following are most frequent, mentioned in the order of their frequency of occurrence: Magnetite, gold, ilmenite, garnet, zircon, hematite and chromite.

It soon became evident that the element zirconium and the element titanium in the form of the minerals zircon and ilmenite, could be obtained in any desired quantity from these sands; that supplies of monazite, perhaps greater and more easily availed of than those already known, are to be had at small cost from these sands, and that occasionally platinum and chrome iron ore can also be extracted from them at the same time, and at low cost.

The method of analysis varied of course to some extent with the complexity of the sand, but in general we found it simplest and most efficient to pass the sand dry, and well screened, over a Wetherill magnetic separator. With the machine used by

us, a current of 0.2 of an ampere developed sufficiently strong magnetic force to extract all of the magnetic iron ore. Increasing the current strength to 0.8 of an ampere, and passing the sand through the machine again, all the chrome iron ore and ilmenite were extracted. A current of 1¼ amperes in this powerful machine was sufficient to extract all of the garnet, and by successively raising the current strength, the magnetization was sufficiently raised to extract olivine and hypersthene, and finally, at a current of 3½ amperes, monazite was extracted.

In this there is nothing new, as this has been the process for the extraction of monazite in Western North Carolina. Left behind were gold, pyrite, zircon, quartz, and several other non-magnetic minerals. As to platinum, nearly all of the grains were magnetic at one current strength or another, and left behind as absolutely non-magnetic was only the iridosmine, which frequently made up half of the mixture of platinum metals.

For further separation of such minerals as were left together in this magnetic separation, such as ilmenite and chromite, separation by a static electric machine, such as the Blake-Morscher, proved particularly easy, so that the combination of these two electric processes will probably be sufficient for the separation of these sands as far as experimental work is concerned, and where the sands are sufficiently concentrated, it is probable that such a combination would be efficient for commercial work, but in the condition in which these sands are obtained from the placer miners, some different and cheaper method is essential.

The greatest need for the "black sand" industry was cheap machinery by which the precious metals, platinum and gold, and if possible, also the other valuable materials, such as monazite, zircon and chrome iron ore, could be cheaply extracted with the platinum and gold. For this purpose the Survey invited makers of concentrating machines to send them for exhibition at the Portland Exposition and to operate them on the "black sands," as a field for friendly competition. The fortunate responses to this invitation included various mechanical shaking tables, such as Wilfley, Pinder, Woodbury and Christonsen tables, all of which proved able to effect the desired sepa-

ration. These tables consisted in a surface covered with linoleum on which were tacked many very small, narrow strips of wood to form riffles. Over this flowed the water, carrying the sand just as in sluice boxes, and the sand was caught in the riffles and without some additional factor, there would obviously have been no gain over the previous separation in the sluice boxes from which these sands had been taken. But in addition a shaking motion, or rather a blow, is given to the side of the table tending to throw the heavier materials to one side. The efficiency with which the platinum and gold were separated by such tables is almost beyond belief, and sands which had been absolutely refractory gave up their platinum and gold even when it was so fine as to serve as paint without further grinding. As much as ten to twenty tons per day could be worked on one of these machines and it was possible to carry on the extractions on a commercial scale. Twenty-five samples, each containing all of a whole carload were examined, and hundreds of smaller samples, many several tons in size. While it was impossible within the limited time to examine all the "black sand" localities in the West, still over 1,000 localities were examined and the principal useful minerals separated from those of no market value.

The results show these "black sands" to be a rich field for supplies of various rare minerals now becoming useful. Boise Basin, in Idaho, proved rich in monazite. Nearly every sand examined contained sufficient zircon to make this an object. Chrome iron ore can be obtained in any desired quantity, and by the concentrating apparatus it can be produced in a greater state of purity than any chrome iron ore now sold on the market. As to the uses for this chrome iron ore, any one can study this interesting subject right here in Philadelphia, where the Kalion Chemical Company manufacture potassium and sodium, bichromates for electric batteries, calico dyeing and other industries. The monazite industry has been developed in our midst as nowhere else in the world. It is now obtained from North Carolina and brought to Gloucester, New Jersey, where Dr. Waldron Shapleigh, with chemical skill which has never been equalled, developed methods for its decomposition and for extracting thorium from it on a commercial scale. Since his unfortunate death the work has been carried

on most efficiently, and the chemical process still further developed by the exceptional ingenuity and efficient skill of your Dr. H. S. Miner. This work has even led to the discovery of new elements, among the by-products obtained from these decompositions of monazite and zircon.

Another mineral which was found in practically every sand examined was ilmenite, the oxide of iron and titanium. Until lately this mineral has had the most unfortunately bad reputation among metallurgists. Its presence to any great extent in an iron ore deposit rendered the ore so refractory that we know of many deposits of rich iron ore condemned on account of the presence of this objectionable element. It leads to the formation of salamanders, that is, infusible masses, in the blast furnace. While many blast furnace misfortunes have doubtless been erroneously charged against the presence of titanium in the ore, there is no question but what it has proved a nuisance to the iron master. It is a significant fact that within the last few months this almost useless metal, heretofore only used to give a yellowish tint to porcelain in the manufacture of artificial teeth, has now come to have much commercial value for furnishing titanium chloride as a mordaunt for the calico printer. Further, other uses in connection with electric lighting have lately been found for mixtures of magnetite and ilmenite, so that altogether ilmenite must in the future be reckoned as a useful mineral and lose its reputation as a metallurgical nuisance. Enough has been said to indicate that in the treatment of our "black sands" one must give attention to the by-products which can be obtained together with the precious metals.

As to these precious metals themselves, naturally gold is most important as to metal value, and the yellow metal will always rank highest in popularity with the miner.

It needs no lecture to furnish inducement to the placer miner to save the gold to the last possible extent, but on the other hand, it is an astonishing fact that enough *platinum* has been thrown away annually in placer gold mining in the West to more than supply all the needs of the United States. To be sure, the amount of this platinum thrown away at any one place is particularly small, amounting usually to one-tenth or

less of the gold which is also thrown away, due to its intimate mixture with the grains of "black sand," and the difficulty of separating it therefrom. But this platinum is an essential in many industries. It is a metal of which we must have a certain quantity, even when a large demand and lack of supply sends the cost temporarily to over $40 an ounce, as has been the case within the last year.

Within the short space of four years the element platinum has taken on an entirely new significance as an industrial metal. This can be concisely expressed by the fact that its price has increased practically ten-fold in that time. If we consider for a moment this change in the position of platinum relative to other metals, it will develop clearly the important characteristic of this element and its kin-folk. The remarkable change in the price has included a human element of great significance, and one which we shall consider first in order to dispose of it. Let us consider, therefore, as more interesting to this audience (although not more significant perhaps in its effect upon the metal) the features in the price fluctuations in this metal due to human agencies. It should be recognized that the association of dealers in platinum, including the French Platinum Society, the firm of Hereaus in Hanau, Germany, Johnson & Mathey, in England, and one or two other smelters of platinum, have undertaken to assume charge of the destinies of platinum, and temporarily at least, have placed its statistical position where it never would have been without their connivance. This association is generally supposed to have the support of several other concerns, more or less prominently connected with the distribution of platinum to the various industries that consume it. For several years there has been apprehension of a platinum famine. The reasons for this apprehension are variously stated by diverse interests. On the one hand the plausible statement is made that during the Russian-Japanese war the miners in the Urals were pressed into military service, with none to take their place in the platinum mines, and later the internal disturbances in Russia, have been equally disastrous to the platinum production. This argument is combatted by rival interests, who maintain that the production has not diminished in Russia, but that the platinum merchants have assumed control and have hoarded the supply. For example,

I have talked with scientific gentlemen who have seen an amount of platinum hoarded in Hanau very much in excess of the stock naturally required by the owning concern. It is argued further that the demand for platinum has largely extended, but no one is ready to point out the cause of such extension except the natural growth of the industries to which platinum is incidental. It seems to the speaker that the great and sudden changes can only be explained by adding the two causes together, and still the sudden jump in value is almost marvelous.

Platinum production has always been essentially a placer mining matter, and it is only natural to expect quick exhaustion of the most profitable ground and increasing scarcity of the product. It is a marvel that Russia has kept up its production as well as it has, and we must recognize a minor benefit to mankind from the manifestly barbarous practice of consigning political prisoners to exile in Siberia, where they furnish the cheap labor for platinum mining. Apparently the placer deposits which have yielded platinum in the Ural Mountains of Russia have at last become so impoverished that the determination is evident among platinum users to look elsewhere over the world for future supplies. In this quest South America has been turned to again and again in the hope of utilizing the placer deposits of the West Coast. The supply there is evidently second only to Russia in the amount of platinum which could be furnished. Each time, however, development of these deposits has been repelled by malarial fever and by governmental interference, and after attention to platinum in Australasia the seekers have now turned determinedly to the West Coast of the United States. Our two years' experience in investigating this region leads to the belief that for practical purposes our West Coast must be considered as the most favorable hunting ground of the future for the platinum metals, and we hope also that the deposits of copper and iron sulphides in which platinum and palladium have been found lately in the course of the extraction of other metals, will yield a supply of platinum for the distant future when our placers have followed the course of Russia and become poorer more rapidly than the facilities for extraction shall have increased.

A glance at a map will show where platinum is found in the West and why.

Let us then consider the conditions of occurrence of platinum in California and Oregon. We should remember that practically the first mention of platinum in the United States was by Blake, who first found it as almost a fine powder in the fine heavy sands of the Pacific Beach in the neighborhood of Port Orford, near the mouth of Rogue River, Oregon. Blake made quite a study of the platinum, and found that it was usually about one-tenth as common as gold on the various beaches, and the proportion of platinum with gold was found to be greater going farther north along the beach into Coos County. Not long after this Silliman investigated the heavy sands in Butte County, California, especially near the village of Cherokee. He noted platinum and its associated metals, iridium and osmium, and this village of Cherokee has remained a classic spot for the study of platinum and its allies in black sands. Later studies by various authorities have revealed platinum in many localities in Del Norte, Humboldt, Siskiyou and Trinity Counties in California, and directly joining these counties in Oregon platinum is found to a very marked extent in Josephine and Jackson Counties as well as in Curry and Coos Counties already referred to. In order to make anything like a complete picture of the occurrence of platinum in the West, we must think also of the Pacific beaches in Washington, and of a very remarkable platinum locality near the head waters of the Similkameen River, B. C. Platinum, together with josephinite, is quite plentiful on the head waters of the Fraser River, particularly near Lilloeet, in British Columbia. In the southern part of California, the shores of Monterey Bay, and the southern part of San Luis Obispo County and of Santa Barbara County are also significant platinum localities. In connection with the locality already referred to in Butte County, it should be noted that platinum has been traced northward to the northwestern corner of Plumas County, and southward as far as Marysville, in Butte County. If, therefore, we think of one very large locality comprising the four southwestern counties of Oregon and extending south to include the four northwestern counties of California, and with a detached deposit in Butte and Plumas Counties and another in Mon-

terey, San Luis Obispo and Santa Barbara Counties, still another stretch along the Washington beach, and another in Southern British Columbia, we can fix in our minds the principal hunting ground of the future for this precious metal, lately priced at twice the value of gold. To be at all complete, we must point to minor localities in California; along the Snake River near its mouth, in Idaho, sporadic occurrences in Montana, and unexplored suggestions of large deposits in southeastern and southwestern Utah. In the effort at completeness we must also call attention to some gravels in the neighborhood of Baltimore, Maryland, and to the fact that platinum has been discovered minerallogically in North Carolina; in a glacial drift on Lake Champlain, and that commercial quantities have been found in Sudbury, Canada, and a deposit much like this Sudbury material also gives commercial promise southwest of Laramie, Wyoming.

Before we confuse ourselves with these minor occurrences, it is well to fix our attention on the California and Oregon main fields by a glance at the geological map of California. These maps show, by certain numbers, the principal rocks noted in different localities. The more we study this map the more evident it will become that the placer mines containing platinum are rich in so-called black sands. These black sands are for all practical intents and purposes a mixture of chromite (as the most *significant* factor), together with magnetite, ilmenite, and a few other heavy minerals. The practically universal occurrence of chromite in such of the placers as yield platinum enables us to trace this heavy sand back practically every time to large masses of serpentine. This serpentine, you will notice, particularly characterizes the coast range by remarkable persistent series of large masses extending north and south through the counties of California and Oregon where we have already noted platinum as being plentiful. I wish to bring out particularly clearly not only the fact that platinum is allied with these particular basic magnesian rocks, but that the platinum sand washed from these sands is peculiarly characterized. Practically always it carries iridium and osmium. Usually these two elements in the form of iridosmium form a considerable percentage of the entire amount of platinum metals. Occasionally they exceed the platinum in frequency. Palladium

is infrequent. Ruthenium, be it remarked, is a characteristic of the platinum found in one locality, Josephine County, Oregon. It would be easy to state the region of a given specimen of platinum sand, if this ruthenium is noted in it. The sand in other respects, however, is like the mixture of platinum metals found elsewhere throughout this region. It is well at this point to leave the Pacific Coast for a moment in order to contrast this occurrence with those in Sudbury, Canada, and the analogous deposit in the Rambler copper mines, and the also analogous occurrence of platinum in copper ores in southwestern Utah. In the Rambler mine iridosmium is as yet unknown. Palladium and platinum prevail, and so far not in metallic state, but as arsenides. Further, where the platinum and palladium thus occur as arsenides in deposits of sulphide of iron and copper, the associated rocks are not olivine and its derivatives, but vary so greatly among themselves as to bear no simple relation to the occurrence of platinum in them.

The most frequent heavy mineral in these "black sands" is magnetic iron ore; this not only almost everywhere, but the percentage is frequently high. In eighty samples taken from the Pacific beaches, from San Diego in Southern California, to Cape Flattery, Washington, the magnetic iron ore averaged twelve per cent. Naturally, our sands were usually concentrates, and this is not intended as an average for the whole coast. Still, several samples showed forty per cent. of magnetic iron, and four car-loads from the mouth of the Columbia River contained thirty-three per cent. Similarly, the sand at the mouth of Gray's Harbor, Washington, was unusually rich in magnetic iron. In fact, a dozen or more places were found where the iron ore could be separated in pure form at a total cost on board barges in still water for less than $1.00 per ton and containing over sixty-five per cent. metallic iron.

It should be recalled that your own authority, John Birkinbine, for years the honored president of this Institute, pointed out years ago this important source of iron for the Pacific States.

Lest some objection might be made to the quality of this iron ore, it should be pointed out that with the cöoperation of the Portland General Electric Company, this iron ore was smelted in the ordinary arc electric furnace of the Heroult type and satisfac-

tory pig iron made, which showed unusual toughness, due to one-half per cent. titanium which it contained.

From what has been said, surely you will recognize that the time has come in many branches of our mineral industry where, instead of simply using our richest mineral deposits until they are exhausted, we are already supplementing them by enriching humbler low grade materials and making them equally efficient. And in the concentration process it will be well always to keep a watchful eye for useful by-products.

The great gain in this new era of concentrating processes rests not alone upon the fact of adding greatly to our available mineral wealth, but most essentially in the beneficial change of our mining industry from the pursuit of hidden treasure to the encouragement of mechanical and chemical advance in applying waste materials to the benefit of humanity.

MEASUREMENT OF ICE-BOUND STREAMS.

The laws governing the flow of rivers that are frozen over have recently been investigated by members of the United States Geological Survey. A paper, entitled "Determination of Stream Flow During the Frozen Season," by Messrs. H. K. Barrows and R. E. Horton, has been published in which the effort is made to formulate methods for estimating the flow under such conditions, and important recommendations as to methods are made.

Estimates of the flow of rivers are made in all parts of the country. To a great extent these are based on daily gage readings and numerous current-meter measurements. In the northern and central parts of the United States the streams may be closed by a more or less permanent ice cover for a considerable portion of the year. This period varies from nearly five months in the extreme north to a few weeks or less in the Central and Atlantic States. The methods in use for estimating flow under open-channel conditions have become well defined, and the limits of accuracy are known to be reasonable. On the other hand, the study of the flow of streams under ice cover is but just started, and in order to systematize the accumulation of data and to provide the material in convenient form for future use it is desirable that certain general methods be followed.

As estimates of flow, to be of conclusive value on streams utilized for water power, must embrace these winter periods of low water, it is easy to see the importance of this inquiry.

According to a U. S. consular report, thorianite, one of the rare minerals, has been discovered in a riverbed of Ceylon. Quantities of the mineral have been mined, and have sold at $8500 per ton.

THE CEMENT INDUSTRY IN 1906.

The following statement, issued by the United States Geological Survey, shows the approximate production of hydraulic cements in the United States for the calendar year 1906.

This statement is exact within a small fraction of one per cent. and is issued in advance of the annual report on the production of cement which is now being prepared in that Bureau. The returns on which it is based are complete with the exception of those from four small plants.

The total production of all kinds of hydraulic cement in 1906, including Portland, natural-rock and Puzzolan cements, was 50,027,321 barrels, valued at $54,015,713.

Of the above total amount of cement manufactured in the United States in 1906, 45,610,822 barrels were Portland cement, with a value of $51,240,652; 3,935,275 barrels were natural-rock cement, with a value of $2,362,140; and 481,224 barrels were Puzzolan cement, valued at $412,921.

Prices were good in 1906, and showed an advance over those of 1905. The total production of cement in 1905 was 40,894,308 barrels, valued at $36,012,189. Comparison of totals for 1905 and 1906 shows an increase in 1906 of 9,133,013 in production and $18,003,524 in value.

QUARRYING ICE IN SWITZERLAND.

The introduction of electric railroads into Alpine districts has been the means of establishing a new industry—namely, the quarrying of glacier ice for distribution in large cities. It would appear that certain of the Swiss communes have been able to grant concessions of their glaciers for this purpose, and considerable sums have been expended in constructing ice slides or V-shaped troughs, in which the blocks of ice, often of large size, blasted out of the glacier, are transported to the vicinity of the stations for conveyance in carefully refrigerated cars to Lyons and other large cities remote from the Alps. The method of blasting with black powder so as to avoid the discoloration and soiling of the ice, and the engineering ability displayed in erecting the slides and in providing sufficient friction by means of curves to avoid excessive speed in the downward journey of the ice blocks, are spoken of as examples of considerable ingenuity and skill.—*Metal Worker, Plumber and Steam Fitter.*

CLAYS OF THE ST. LOUIS, MISSOURI, DISTRICT.

In the year 1905 St. Louis produced and sold about $5,000,000 worth of clay products—approximately one-thirtieth of the entire output of the United States. A very large proportion of these were made from fire clays, dug in a single section within the city limits. A brief description of the clay resources of the St. Louis district, prepared by N. M. Fenneman, is included in the United States Geological Survey's "Conributions to Economic Geology, 1906" (Bulletin 314), just published and obtainable on application to the Director of the Survey, at Washington, D. C. The paper contains also references to available beds of shale and brick clay near the city and a map showing the locations of the fire-clay mines.

(Stated Meeting held Thursday, May 9th, 1907.)

The Goldfields District, Nevada.

(ABSTRACT.)

The new camp of Goldfields, in Nevada, was revisited in November of 1904 by Mr. J. E. Spurr, of the United States Geological Survey. This district lies about 23½ miles south of Tonopah, and was located late in the spring of 1903. Shortly after the discovery of gold in this district it was visited by Mr. Spurr. A little work was then in progress on what is now known as Columbia Mountain, but up to that time no good strikes had been made. In January and February, 1904, however, rich finds were made in certain spots south of Columbia Mountain. During the next year the district acquired an approximate population of 6,000. The town of Goldfields has sprung up, and a number of smaller adjacent camps have been established.

The district is bounded on the west in part by a lava-capped mesa, the erosion of which has laid bare the underlying gold-bearing rock. The auriferous region is characterized by numerous low, irregular ridges standing out from the lower and more nearly level surface. These ridges owe their origin to hard reefs of quartz which form their crests. Their resistance to erosion has left them protruding thus above the general elevation, and in these quartz reefs the auriferous deposits are found.

Columbia Mountain is the most prominent of these ridges, and some notes on its geology were made public by Mr. Spurr, in Bulletin 225, published by the United States Geological Survey. Near the south end of the ridge the rock is largely alaskite (an igneous rock consisting of quartz and feldspar), which is sometimes of granitic structure, and sometimes very fine-grained, even resembling quartzite. White mica or muscovite is sometimes present, and pure quartz veins of dikes, of similar origin, also occur. These alaskitic rocks are intrusive into a dark siliceous rock (jasperoid) which is probably the result of the silicification of an original limestone. We may suspect that this limestone was paleozoic, and that the alaskite is post-Jurassic. On the north end of the Mountain the

rock is a very much altered rhyolite; in this rhyolite are broad masses of white to purplish and reddish cherty quartz, which extend irregularly in a northerly direction. This quartz is simply a highly silicified rhyolite. The silicified areas have ill-defined walls, and the highly mineralized portions which they enclose are very irregular.

On his last visit Mr. Spurr took notes over an extended area, and made interesting additions to his knowledge of the geology of the district. The area of known ore bodies has spread so far beyond Columbia Mountain that values are now found over an area several miles square. The most productive area measures two to two and a-half miles in either direction. The chief mines at the present time are the Jumbo, the Combination, the January, and the Florence, all grouped together about a mile south of the southeast end of Columbia Mountain. Some nine miles southeast of Columbia Mountain is the Diamondfields group, including the Vernal, the Quartzite, and Black Butte, from which shipments have also been made. Other ore deposits have been developed in various parts of the field.

The rocks in that part of the field visited by Mr. Spurr on his last trip (the region of Columbia Mountain, Diamondfields, and the Jumbo group of mines) were found to be almost entirely volcanic, consisting of rhyolites, rhyolite tuffs, andesites and basalts, all probably of Tertiary age. The alaskite and jasperoid of Columbia Mountain are hardly represented in the surrounding district. The predominant rocks are abundant rhyolites and andesites, while basalt is rare. One andesite examined microscopically from near the Tonopah Club is a hornblende andesite, resembling the early andesite at Tonopah; a patch of basalt from near the Florence is an augite basalt, like the basalt of Siebert Mountain at Tonopah. The rhyolite resembles the rhyolite of the Gold Mountain district, which lies about four miles south of Tonopah, on the road between Tonopah and Goldfields; and this Gold Mountain rhyolite, again, resembles closely some of the phases of the earlier (dacitic) rhyolite at Tonopah. The relative age of the rocks at Goldfield has not been determined, but it probably corresponds to that of similar rocks at Tonopah.

At Goldfields the ores occur in both rhyolites and andesites, showing that mineralization occurred subsequent to the erup-

tion of both lavas. At Gold Mountain the deposition of the ores evidently followed the eruption of the rhyolites, and at Tonopah the eruption of the earlier (dacitic) rhyolites was succeeded by a period of mineralization which produced irregular veins that frequently carry a larger proportion of gold than the locally more important veins formed after the eruption of the early andesite. There is therefore the possibility that the Goldfields deposits are identical in origin with the later series of veins at Tonopah. Indeed there are at Tonopah, in one place at least, mineralized quartz reefs in rhyolite tuffs that have the same peculiar characteristics as the tuffs of the Goldfields reefs; and assays of these Tonopah deposits, as was known to Mr. Spurr two years ago, have shown a moderate amount of gold and no silver.

At Goldfields there are no definite veins. The outcrops of the quartz bodies are irregular, straggling, branching, and apt to disappear suddenly. Neither were any definite systems observed, though further study might reveal them. There seems, however, to be a tendency to elongation in a northerly direction. The outcrops may even be nearly circular, or crescentic, and frequently they are roughly lenticular and intermittent. The quartz itself is gray and jaspery; it is almost entirely due to the silicification of the volcanic rock in which it occurs. Practically no ordinary crystalline vein quartz was observed.

All indications show that this silicification (and the accompanying mineralization) is the work of hot springs, and that these irregular reefs represent the horizontal sections of columns of rocks traversed by rising columns of hot water. Had the rocks been strongly fractured we should have had veins, like those of the early andesite at Tonopah, which were also probably due to hot spring action; but at Goldfields the lack of such a fracture system resulted in this curious and rather unusual type of deposit. It follows that the quartz reefs will probably, as a rule, extend deeper vertically than horizontally, and so have roughly the nature of columns or pipes.

Although showing disseminated pyrite, the greater part of one of these jaspery quartz reefs contain little or no gold. Microscopic investigation has shown in one case that in such quartz the iron of the pyrite is probably mainly indigenous,—that is, that the iron sulphide has been formed by the action of

sulphur contained in the hot spring waters, upon the iron silicates contained in the hornblende and biotite. This explains the absence of gold, as the pyrite has the same origin as the barren pyrite near the ore bodies in the country rock at Tonopah. Within some of the barren reefs of silicified rock at Goldfields, however, prospecting has sometimes discovered portions containing gold, even in large quantities. Such portions are usually lenticular or irregular, like the main quartz reefs, and they are not easily distinguishable from the barren quartz, except by panning or assaying; but it seems probable that these sheets are the real ore deposits, and that the mass of the reefs constitutes merely a siliceous jacket or casing, such as is shown to surround ore bodies in some other parts of the world. While this siliceous casing may be twenty-five or thirty feet wide, the auriferous portion may be only one or two feet; and the form and extent of this portion become evident only after the ore has been extracted. It is then seen to have a definite channel-like shape, often more regular than that of the whole outcropping reef, though it has usually a limited extent in the direction of its greatest elongation. It seems probable that these pay shoots represent the main channels of hot-water circulation, while the siliceous casings are the result of the water soaking through the adjacent rock.

The ores are often of very high grade. As an extreme example may be noted a shipment of 14¼ tons from the Sandstorm (Kendall claim), which when worked in a stamp mill yielded $45,783 net, while the tailings still contained about $1,000 to the ton. From the McKane-Bowes lease on the Jumbo, $600,000 was taken out in five months, from a space of 100 feet horizontally and 200 feet vertically on the sheet. One small shipment of 917 pounds of ore from this lease gave gross returns of $4,766. The whole production of the camp has been from ore which may be roughly estimated as averaging $200 to $300 per ton, or more. The values are all in gold; silver is usually practically absent; although the shipping ore from the Combination mine contains from one to three ounces.

It is important to consider the origin of this rich ore in order to make prophecies for the future. Most of that which up to the present time has been extracted has been oxidized ore. The ores are mixed sulphides (usually pyrite) and oxides, clear up to the surface. The oxidized material, which follows cracks

and seams, is usually several times (sometimes several hundred times) as rich as the unoxidized portion. The irregular spongy nature of the free gold particles in such oxidized material completes the proof that this gold has been dissolved and redeposited in a concentrated form during the process of oxidation. Iron sulphate derived from oxidation of the pyrites is the probable agent. A peculiar yellow coating pointed out to Mr. Spurr as the best sign of values in the oxidized ores, was shown by Dr. Hillebrand to be a basic ferric-alkali sulphate, containing both sodium and potassium,—perhaps jarosite. Other sulphates, such as alum and gypsum, are abundant. These oxidized ores are prepared for shipment by screening, the fines being shipped and the coarse quartz rejected.

As the water level at Goldfields is usually high for this desert country (water having been encountered in several shafts at 130 to 200 feet) it is plain that this oxidized ore is only a temporary supply. In the Combination and the Florence, however, sulphide ores of very high grade have been found below the oxidized zone. In these mines a dark gray copper-bearing mineral, which is very rich, is most intimately connected with the gold. A specimen from the Combination, analyzed by Dr. Hillebrand, proves to be a sulpho-salt of copper, antimony, and arsenic, which, so far as qualitative composition goes, may be tetrahedrite. Tellurium is also present in this ore, and the same element has been reported elsewhere in the district. Therefore the sulphide ores also may be very rich. Moreover, while the difference between the oxidized and the unoxidized portions of the ores within the zone of oxidation is in general so great, certain sheets occur, as in the January and the Jumbo, where the unoxidized quartz in this zone is of extremely high grade. Such ore appears to be mostly pyrite, but in view of the fact that tellurium is found in the district, it is very possible that gold telluride may be present. It therefore appears probable that the rich oxidized ores owe their richness not primarily to concentration during oxidation (though this process has certainly been very important), but to the existence of rich antecedent (sulphide) ore.

Concerning the origin of these sulphides, it is probable that some, so far as can now be seen, are purely primary, while others have been formed subsequent to the main silicification of the reef, as in the Combination mine. Here the rich aurifer-

ous sulphides have formed in a broken zone (breccia-zone) in the silicified barren reef, and occur as seams, and often as coatings in the pebbles in the breccia. The question arises, however, as to whether the subsequent mineralization was the result of descending or ascending waters. Concerning this the evidence is not conclusive, but there is no sufficient evidence that these rich ores have been concentrated from the lean antecedent quartz mass. The presence of elements like arsenic, antimony, and tellurium in the subsequent sulphide ore suggests a deep-seated origin.

Besides the elements mentioned above, bismuth occurs in the ore. In the January it occurs in the oxidized ores in the form of silvery scales, which is, as determined by Dr. Hillebrand, bismuth, perhaps the oxide bismite. In the Combination long needle-like crystals have been found, which, according to the manager, Mr. Collins, give the chemical tests for bismuth sulphide, bismuthinite. The silvery mineral above noted is sometimes found, in the January, arranged in long rod-like forms, and these are probably pseudomorphs after the sulphide. In the January this silvery mineral is usually, but not always, an indication of rich ore. Barite is a common mineral in all these deposits, but is not abundant.

The indications are, therefore, not unfavorable to the continuance of high-grade, or at least good-grade ores, down to considerable depths. There is, however, as has been already demonstrated by exploitation, no continuous regularity to the ore shoots, whether sulphide or oxidized. They are curving, irregular, and often lenticular, but it may happen that below a shoot which has come to an end, another shoot may be found occupying a slightly different relative position, or even overlapping the first. Similarly, the main quartz masses as a whole can be expected to show little regularity in depth; they may increase in size, or diminish,—or even disappear, at least temporarily.

Erratum.

Art. Obholzer, Extract of Report on the Methods Used to Avoid Piping in Steel Ingots, &c., July, 1907. Page 6, line 1, *for* inches *read* feet.